METAL PROGRESS

May - 1945

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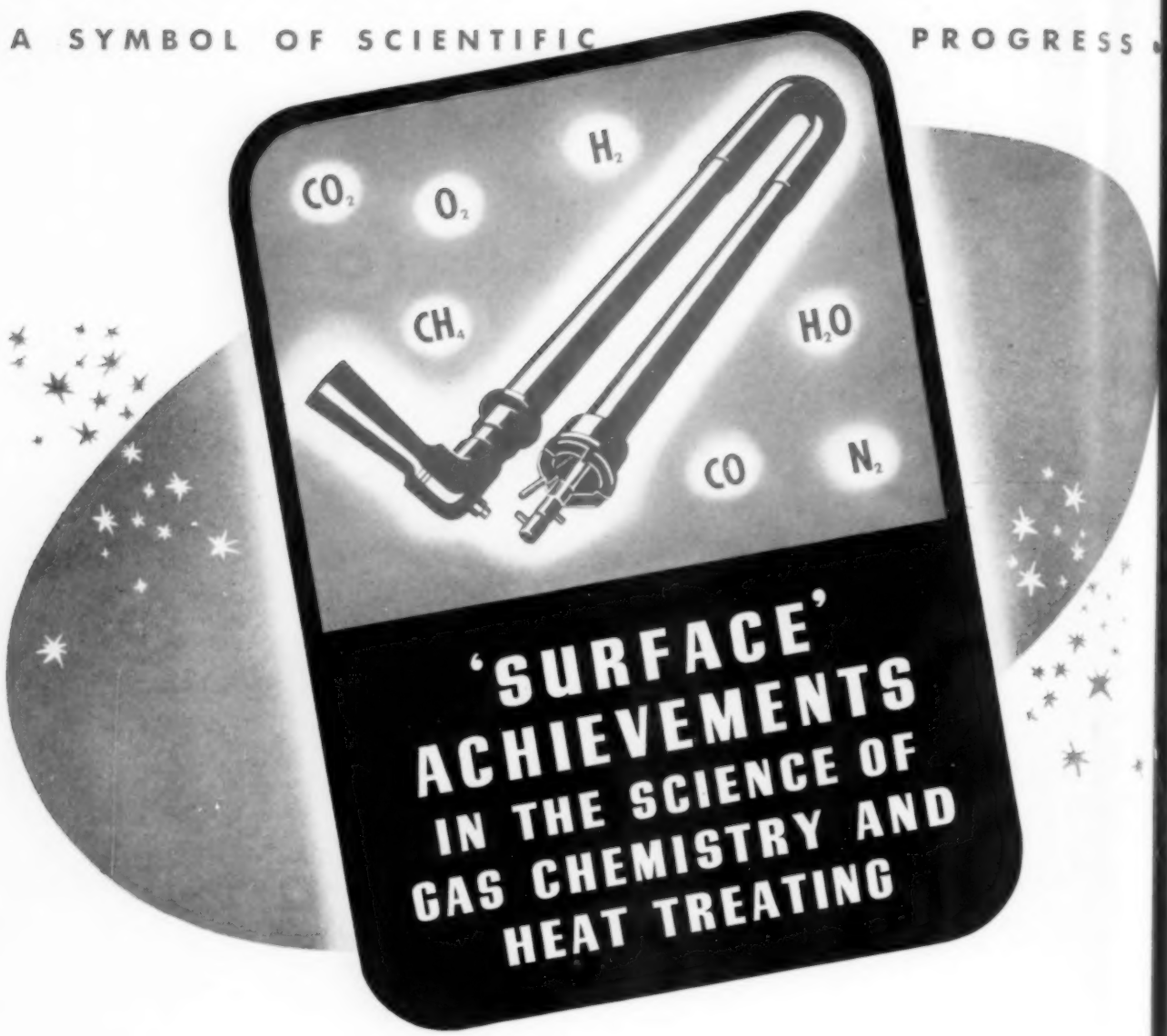

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METAL PROGRESS

Vol. 47 **May, 1945** **No. 5**

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MEASUREMENT OF CASE DEPTHS BY MARTENSITE FORMATION

By E. S. Rowland
and S. R. Lyle
Metallurgical Department
The Timken Roller Bearing Co.
Canton, Ohio

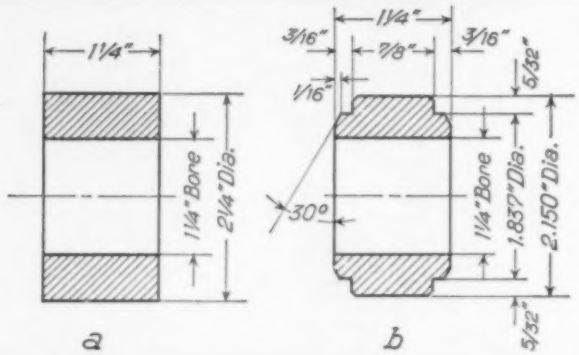
ANYONE engaged in production carburizing requiring close control of case depth will attest the trouble and uncertainty surrounding its accurate estimation. Microscopic measurement on polished and etched sections yields a fairly accurate figure for the total penetration of added carbon, but not all of this depth is useful case since part of the gradation zone is of low hardness. Consequently attempts are made to measure the depth to the 0.40 to 0.50% carbon level on hardened microsections by observing the color change under a ten-power Brinell microscope; this leads to quite inaccurate results because of etching variables and differences in judgment among observers.

A more accurate method in use in the Timken Roller Bearing Co.'s plant for several years is as follows: Finish the hardened and ground section on 240-grit metallographic paper. Etch in a cold 3% solution of HNO_3 in alcohol for 1 to 4 min., depending on the size of the specimen and the case depth. Read the depth to the brown-to-blue color change with a Brinell glass. The result correlates reasonably accurately with the position of a 0.50% carbon concentration. Unfortunately, the accuracy of the method depends upon the skill of the operator; considerable training and experience are required before even reasonably reproducible results can be obtained.

The use of "carbon test rings" in our plant practice for the accurate determination of carbon gradation has been described previously by Gable and Rowland in the 1937 ⊕ Carburizing Symposium (p. 280) and its details need not be repeated. Figure 1 shows at left the dimensions of the machined ring which is carburized with the charge and later cooled in mica. After preliminary machining of the flat faces to ⅞-in. width, ten or more 0.005-in. cuts are removed from the outside diameter and the ring finally assumes the shape shown at the right of Fig. 1. Chemical analysis of these chips affords an accurate method of determining maximum carbon concentration in the case, carbon gradation and case depth. Although accurate as a research method, the procedure is time consuming, expensive and, in general, inapplicable to the measurement of case depth on production parts.

In some recent experimental work on the "martempering" of carburized parts, an extremely sharp line of demarkation was noticed in the gradation zone of etched microsections between the higher carbon layer which did not transform into martensite at the quenching bath tempera-

Fig. 1 — Carbon Test Ring (at Left) as Inserted in the Carburizing Charge, and After the Successive Layers Have Been Machined Off for Chemical Analysis (After Gable and Rowland)



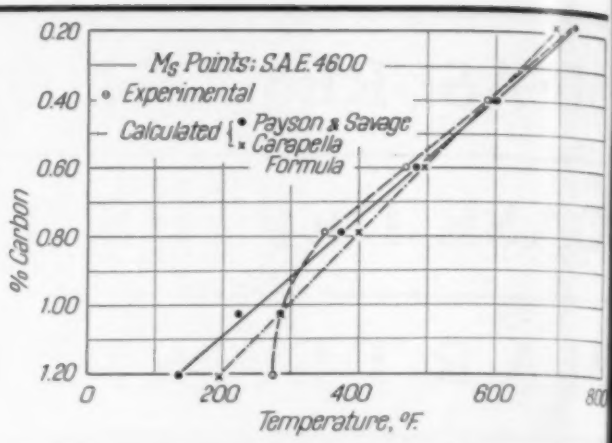


Fig. 2 — Martensite Points for S.A.E. 4600, Determined Experimentally and Calculated from Chemical Analysis

ture and the lower carbon material which did. The sharpness of this limit of martensite transformation is shown by the fact that different observers were immediately able to agree on its position within one-half a Brinell glass division (0.002 in.). By taking advantage of the effect of carbon content on the martensite point the possibility was recognized that, by manipulation of the quenching bath temperature, the case depth could be accurately determined at any desired carbon level.

The experimental results described herein comprise the development of this idea to the measurement of case depth in S.A.E. 4620 and its wartime, low-nickel substitute R.B.E.C. 4720, both used in the manufacture of roller bearing parts. All remarks are to be construed as applying *solely* to these two analyses. (An experimental program is under way to see whether the method can be extended to other carburizing steels and to measure the decarburization of high carbon steels, and the results will be reported later.)

In its simplest form, the proposed method consists in austenitizing a small section cut from the carburized part. Time and temperature of this heating must be sufficient to put all the carbon in solution up to the desired level of measurement. (That is to say, if we are interested in the 0.50% carbon level, the austenitizing temperature must be somewhat above Ac_3 for the given alloy steel containing 0.50% carbon.) After austenitizing, the piece is quenched into a bath held at the martensite point of that analysis. The sample should be held in this quenching bath long enough to permit a satisfactory degree of tempering of the martensite formed during the quench (for contrast) yet not long enough for the higher carbon austenite to decompose, isothermally, into bainite. (If carbon levels still above their martensite points transform in this manner it would show as an apparent reduction in the case depth.) The specimen is then water quenched, ground on one of the cut faces, polished, etched in nital, and the case depth at the chosen carbon level may be read accurately with a Brinell glass or under the microscope.

Obviously, in order to use this scheme, it is necessary to determine fairly accurately the martensite points for 4600 and 4700 steels of various carbon contents, and to know that these figures are relatively insensitive to changes in alloy content within the limits of the standard specifications. In obtaining this necessary preliminary information, we used the same series of induction

Table I — Deviations of Measured Depths to Actual Locations
S.A.E. 4620 (C 0.17, Mn 0.57, P 0.014, S 0.016, Si 0.29, Cr 0.17, Ni 1.75, Mo 0.24)

	Carbon Level			
	0.40%	0.50%	0.60%	0.70%
Calculated Ms point, °F.				
Payson & Savage's formula	595	538	481	424
Carapella's formula	588	542	492	442
Higher Ms quench				
Temperature, °F.	590	525	470	395
Deviation, deep case	+0.004	+0.004	+0.003	+0.003
shallow case	+0.001	+0.005	+0.003	+0.004
Lower Ms quench				
Temperature, °F.	570	505	450	375
Deviation, deep case	zero	+0.002	+0.001	−0.001
shallow case	zero	+0.001	−0.001	+0.001

heats of nickel-molybdenum base analyses with varying carbon contents that were employed in the hardenability determinations reported last year by Rowland, Welchner, Hill and Russ, in *Transactions* ASM, Vol. 35, 1945, p. 46. The technique of Greninger and Troiano was used for the martensite points, as follows: Small (0.030-in. thick) specimens of several carbon contents between 0.40 and 1.21% were austenitized 10 min. at 1525° F. in a salt bath; quenching bath temperatures were varied in 20° F. increments and the lowest temperature selected at which martensite did not appear.

Martensite points so determined are shown in Fig. 2. They agree fairly well with the values calculated from the analyses of the induction heats by the methods of Payson and Savage* (*Transactions* ASM, Vol. 33, 1944, p. 261), and of Carapella† (*Metal Progress*, July 1944, p. 108) except for steels containing above 0.80% carbon, wherein excess carbide in the austenite causes the experimental points to deviate considerably from the calculated.

As remarked above, it is necessary to quench the sample from the bath before higher carbon austenite starts to transform. Maximum times in the bath for S.A.E. 4600 are therefore given in Fig. 3, which plots the incubation time for isothermal decomposition of austenite into bainite at temperatures 20 to 30° F. above the martensite points at the various carbon contents. Under the same austenitizing conditions, the incubation period lengthens rapidly as the carbon content increases until excess carbide appears in the austenite, above which point the initiation time decreases, a reversal which is not completely understood.

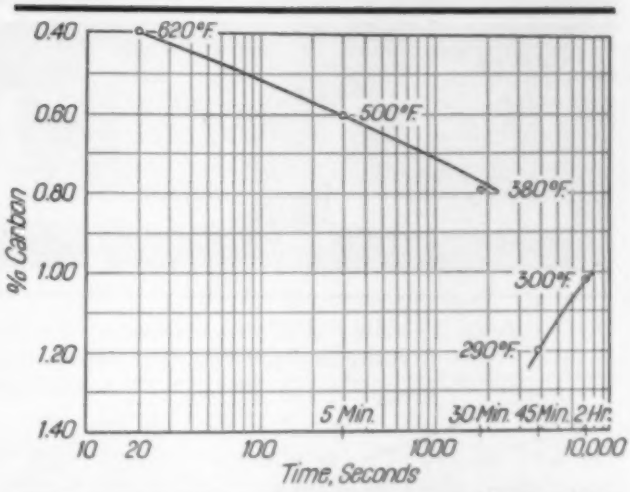


Fig. 3 — Maximum Holding Times for S.A.E. 4600 in Quenching Baths Held About 20° F. Above Ms Points of Respective Carbon Contents

With the above fundamental information at hand, it was possible to proceed with its application to carburized S.A.E. 4620. Carbon test rings (Fig. 1) were prepared from a commercial heat carburized to two case depths and mica cooled. After analyzing the carbon cuts from the outside diameter, small sections were cut from the inner bore, austenitized 15 min. at 1525° F. and quenched to bath temperatures of 590°, 525°, 470° and 395° F., representing Ms points for carbon levels of 0.40, 0.50, 0.60 and 0.70%, respectively (Ms points according to Fig. 2). Holding times were 30 sec. for the three lower temperatures and

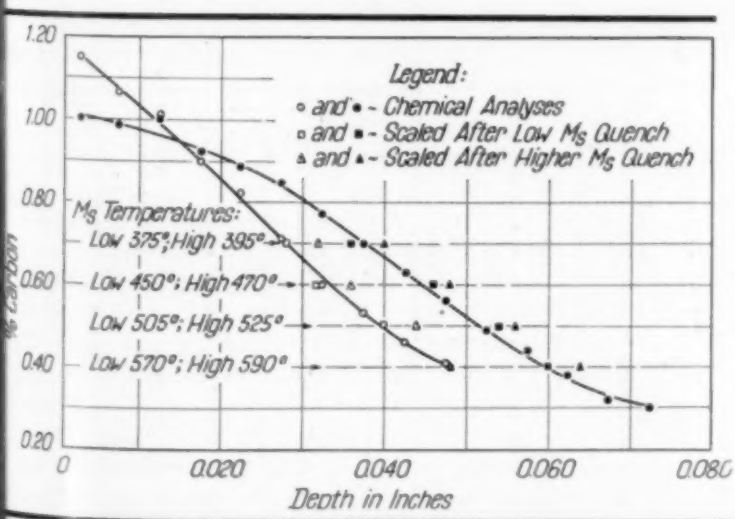


Fig. 4 — Carbon Penetration Curves (Determined by Chemical Analysis) of Two Carburizing Runs on S.A.E. 4620, Together With Depths of 0.40, 0.50, 0.60 and 0.70% Carbon Layer as Determined by New Method

*Payson and Savage's formula is

$$Ms \text{ in } °F. = 930 - 570\,C - 60\,Mn - 50\,Cr - 30\,Ni - 20\,Si - 20\,Mo - 20\,W$$

wherein the contents of the various alloying elements are expressed in percentages.

†Carapella's formula is

$$Ms \text{ in } °F. = 925 \times f_C \times f_{Mn} \times f_{Cr} \times f_{Ni} \times f_{Si} \times f_{Mo} \times f_W$$

wherein the various factors for the alloying elements are as follows:

$f_C = 1 - 0.620\,C$
$f_{Mn} = 1 - 0.092\,Mn$
$f_{Cr} = 1 - 0.070\,Cr$
$f_{Ni} = 1 - 0.045\,Ni$
$f_{Si} = 1 - 0.033\,Si$
$f_{Mo} = 1 - 0.029\,Mo$
$f_W = 1 - 0.013\,W$

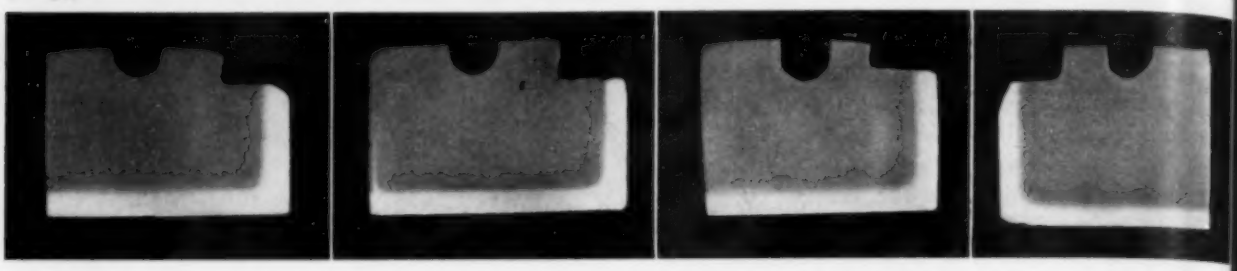


Fig. 5 — Etched Microsections of S.A.E. 4620 Steel Carbon Test Ring Showing Case Depths at 0.40, 0.50, 0.60 and 0.70% Carbon Levels, Magnified 2 Diameters

20 sec. at 590° F. Specimens were then water quenched.

Results of measurements on the etched microsections (using a Brinell glass) are given in the upper part of Table I and the triangular marks in Fig. 4. The curves also show the carbon penetrations from the accurate chemical analyses. While the case depths determined were in good agreement with the analytical carbon determinations, they all lay on the deep side; that is, errors as shown in Table I were all plus. This is due to the fact that the experimental Ms points

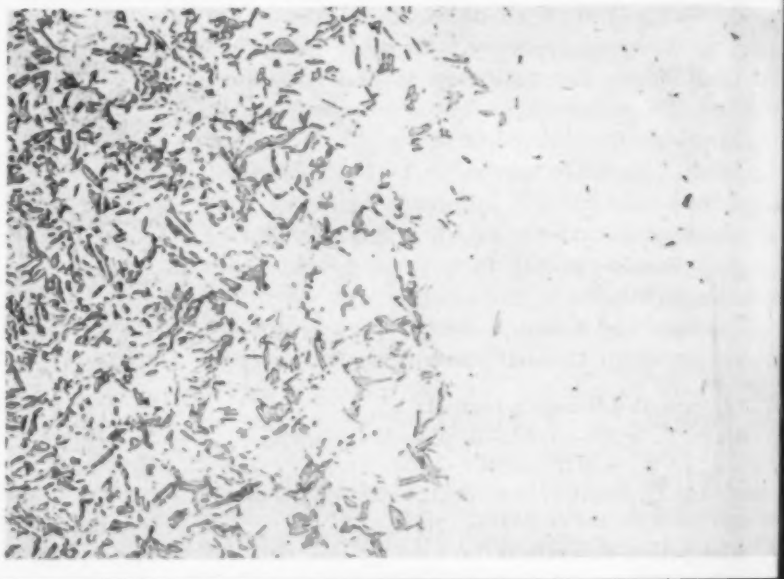


Table II — Deviations in 0.001 In. of Measured Depths in Carburized R.B.E.C. 4720 Steels From Locations Determined by Chemical Analyses

Heat	Case Depth at 0.50% C (Analytical)	Carbon Levels and Quenching Bath Temperatures							
		0.40% C (570° F.)		0.50% C (505° F.)		0.60% C (450° F.)		0.70% C (375° F.)	
		10×	500×	10×	500×	10×	500×	10×	500×
A	0.039 in.	−2	0	+1	+1	−2	−4	−2	−3
	0.048	0	0	−2	−2	−2	−2	−1	+1
B	0.039	−1	−1	−1	−1	−1	−2	−3	−3
	0.045	+2	0	+1	−1	+2	0	0	0
C	0.039	+2	+1	+1	+1	0	−2	−1	−1
	0.046	+3	+1	−2	−2	0	−2	0	−1
D	0.038	−1	−1	−2	−1	0	+1	−2	−2
	0.052	−2	−3	−2	−3	−1	−2	−2	−4
E	0.036	+1	0	−1	0	+1	+1	−2	−2
	0.040	+2	+4	+2	+2	+3	+2	0	+1
F	0.035	+1	0	+1	+1	+2	+2	−1	−1
	0.042	0	−2	0	+2	+2	0	0	−1
G	0.038	+3	+2	+2	+2	+2	+3	+1	+2
	0.047	+2	0	+1	−1	+1	−1	+1	+1
H	0.037	−2	−1	−1	0	−1	−1	−2	−1
	0.042	−3	−3	−1	−3	+1	−1	−1	−1
J	0.036	+1	0	+2	+1	+1	−1	−2	−3
	0.042	−1	−3	−2	−2	−1	−2	0	−2
Z	0.035	+2	+2	+1	0	0	0	+1	−1
	0.046	−1	−3	+2	−1	+1	+1	−1	−1

of Fig. 2 all tend to be slightly high, since the determinations were made at 20° F. increments and the nearest temperature *above* martensite formation was selected for the quenching bath temperature. Furthermore, slight diffusion of carbon may occur in the case during the treatment, tending to deepen it.

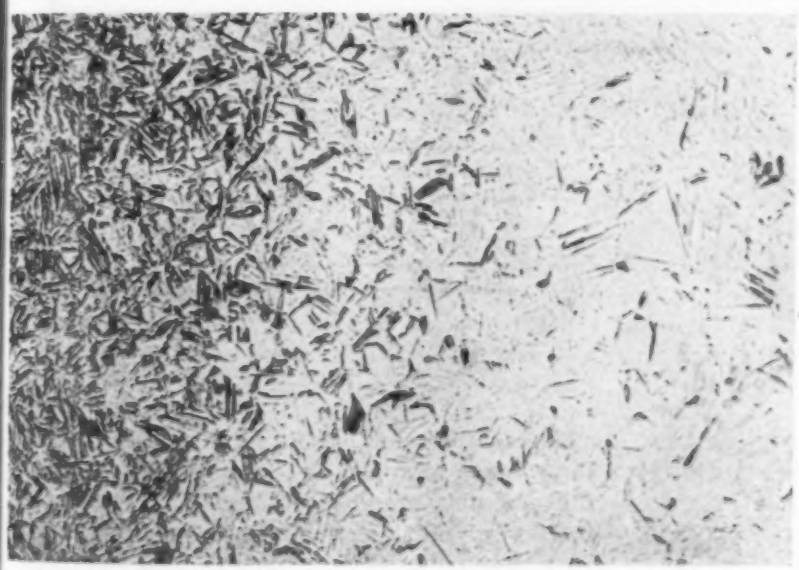
In order to effect a more exact agreement between the case depth measurements and the carbon gradation curves, the quenching bath temperatures were adjusted slightly and additional tests run at a 20° F. lower temperature for each carbon level. As shown in the lower part of Table I and the square marks in Fig. 4, the agreement is now excellent, the maximum deviation being 0.002 in. These results may be slightly overcorrected since the amount of diffusion of carbon occurring in the small samples after carburization stops is at present unknown (but believed to be very small). The results illustrate, however, the precision with which the new procedure for determining case depth to any medium carbon level may be adjusted to fit a known carbon gradation curve and the relative insensitivity of the quenching bath temperature, since approximately 7° F. in Ms temperature corresponds to only a single point of carbon variation.

The sharpness of the line of demarkation at each carbon level is shown in Fig. 5, ranging from the 0.40% carbon level on the left to 0.70% on the right. The abrupt change in microstructure is shown in Fig. 6 at 500 diameters. In markedly banded materials, some sharpness is lost (Fig. 7) but if longitudinal sections are used in these cases and measurement is made to the solid bank of tempered martensite, ignoring streaks of partial decomposition, the accuracy is still very satisfactory. It is believed better to measure with the Brinell glass at 10 diameters magnification than upon the microscope stage at 500 diameters, except when one is studying the case of very homogeneous steels.

Table III — Chemical Analyses of R.B.E.C. 4720 Heats Used for Accuracy Tests

Heat	C	Mn	P	S	Si	Cr	Ni	Mo
A	0.21	0.58	0.014	0.021	0.31	0.24	1.12	0.22
B	0.19	0.60	0.014	0.018	0.31	0.28	1.18	0.22
C	0.20	0.57	0.016	0.015	0.36	0.26	1.15	0.21
D	0.18	0.59	0.015	0.020	0.34	0.30	1.09	0.21
E	0.19	0.57	0.014	0.020	0.31	0.29	1.27	0.21
F	0.21	0.57	0.014	0.018	0.35	0.30	1.09	0.22
G	0.21	0.56	0.015	0.021	0.31	0.28	1.21	0.24
H	0.18	0.58	0.015	0.021	0.31	0.30	1.18	0.22
J	0.195	0.56	0.015	0.018	0.30	0.27	1.21	0.22
Z	0.16	0.59	0.014	0.018	0.32	0.29	1.29	0.25

Fig. 6 (on Left Hand Page) and Fig. 7 — Photomicrographs of Beginning of Transformation in Homogeneous and in Banded Steel, Respectively, at 500 Dia.



Test for Accuracy

As a final test on the accuracy of the method, the above procedure was followed on ten heats of R.B.E.C. 4720 steel at two case depths. The analyses of these heats are given in Table III. The range of calculated Ms points by both published methods at 0.40, 0.50, 0.60 and 0.70% carbon levels are as follows:

	Payson & Savage	Carapella
0.40% C	602 to 609° F.	588 to 602° F.
0.50% C	542 to 550	542 to 552
0.60% C	482 to 495	492 to 502
0.70% C	431 to 438	447 to 450

The calculated Ms points were in such close agreement with those for the S.A.E. 4620 heat (Table I) that we used the corrected temperatures for S.A.E. 4620. Results of the measurements are shown in Table II for both Brinell glass (10×) and microscope (500×). It can be seen that the maximum deviation from the individual

carbon gradation curves (determined by carbon analysis of 0.005-in. turnings) is 0.003 in. with the Brinell glass and 0.004 in. with the microscope. Independent observers have failed to register changes of more than 0.002 in. in any of the figures shown, and then only occasionally. Only the 0.40 and 0.50% carbon levels of steel E represent the results of retesting and these were re-run because the originals obviously represented faulty quenching technique.

The deviation between the adjusted quenching bath temperatures and the calculated Ms points at the four carbon levels has led to an empirical adjustment of the carbon factors in the formulae of Payson and Savage and of Carapella. Since the alloy contents are relatively constant in both steels studied, the factors for all elements except carbon were assumed to be correct and new carbon factors calculated from a curve 20° F. lower than the experimental curve of Fig. 2. Changing the carbon factor in Carapella's equation to $f_C = 1 - 0.72 \cdot \%\ C$, and the same factor in the Payson and Savage equation to $-640 \cdot \%\ C$ produces agreement between the calculated and the adjusted experimental values within about 15° F. for carbon levels between 0.40 and 0.70%. It is emphasized that these altered carbon factors apply only for S.A.E. 4620 and R.B.E.C. 4720 steels, and their worth when calculating Ms points of other steels is not known.

This method of case depth measurement is admittedly in its formative stage and much additional work needs to be done to prove its general value. As applied to these two steels, however, it does seem to show considerable promise for precise determinations of case depth at any carbon level between the limits imposed. For carbon levels much below 0.40% the available holding time in the quenching bath is too short to temper the martensite adequately for effective contrast. At carbon contents of 0.80% and above, the time and temperature required for complete carbon solution are probably great enough to disturb the results by diffusion.

ARC WELDING OF RAIL STEEL

APPLICATION of the common welding methods to rail steel has been investigated at Armour Research Foundation at the instance of the Rail Steel Bar Association, and some of the information obtained on arc welding is published in this article through the courtesy of that association.

Bars and shapes are produced from selected railroad rails of re-rolling quality. Such bars and shapes are used in many markets, such as for agricultural equipment, in the bedding industry, for fence posts and for concrete reinforcing bars. The rails are heated to the proper temperature, de-scaled with water spray and passed through slitting rolls which separate them into three elements, the head, web and flange. Each element then passes to appropriate rolls where it receives the necessary number of passes to reduce it to the desired cross-section. Rolling temperature is such that the finishing temperature is low enough to insure a relatively fine-grained product. The stock is usually air-cooled, resulting in a microstructure combining high yield strength with satisfactory ductility.

By C. B. Haynes
Metallurgist
William H. Graft
Assistant Metallurgist
and Raymond G. Spencer
Chairman, Metals Research
Armour Research Foundation
Chicago

Due to the limiting cross-section of the original rail element and the reduction necessary to form the desired shape, rail steel products are light in section, ranging below 5 lb. per linear foot; gages of the angles, flats and other sections are mostly $\frac{5}{16}$ down to $\frac{7}{64}$ in. These standardized light sections are a distinct advantage

from the standpoint of arc welding, making the process feasible for rail steel where it might not be for similar high carbon steels of heavier section.

Carbon content varies with weight of the original rail but the analysis usually falls within the following limits:

	C	Mn	Si	S	P
Maximum	0.90	0.90	0.30	0.05	0.04
Minimum	0.35	0.55	0.20	....	....

By selecting rails according to weight, the carbon composition can be held within fairly close limits.

The welding of rail steel, then, is the welding of plain carbon steels of analysis varying from the conventional limit of weldability without special precautions, up to hyper-eutectoid analysis.

The usual notched bar impact tests are not desirable for transverse testing of weldments under dynamic loading, because the position of the notch pre-determines the point of break, and for transverse testing, the specimen should be free to break at the weakest point without prejudice. Consequently we used tensile impact tests on bars machined as shown in Fig. 2.

The chemical analyses and physical properties of the rail steels used for weld tests are given in Table I. These physical properties were also determined on test bars prepared from unwelded stock, and dimensioned as shown in Fig. 1 and 2.

A preliminary series of bead-weld tests were made to determine the microstructure in the heat-affected zone resulting from different heat-input rates. These beads were laid with automatic equipment lengthwise on 10x4⅞x⅛-in. rail steel flats, analyzing 0.73% C, 0.80% Mn and 0.03% S. Cross-sections for micrographs were made 5 in. from the starting end of the bead. Heat input was varied from 4800 to 11,000 joules per in. of bead. Martensite was very much in evidence in the heat-affected zone at low heat inputs, and disappeared at above 7500 joules per in. At 11,000 joules per in. the heat-affected structure was pearlitic, matching very closely that of the rail steel, as rolled.

Heat-affected structures near the fusion zone from the above series of bead welds at 5000, 7500 and 11,000 joules per in. of bead are shown in Fig. 3, 4 and 5, respectively (page 915). These sections were etched with 4% picral; martensite appears as white areas.

Bead-bend tests were made of similar sam-

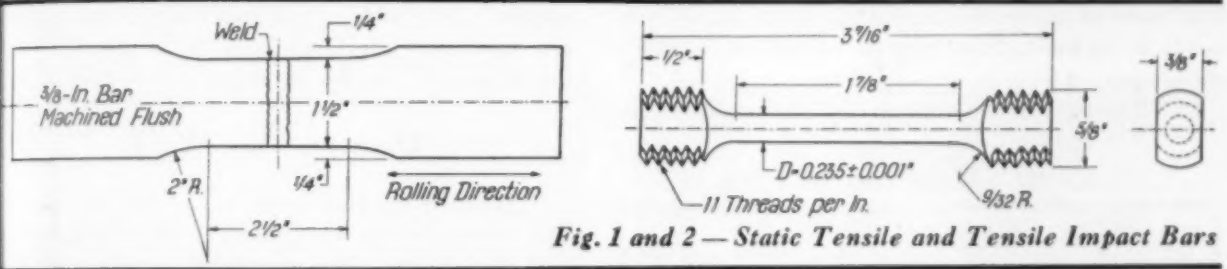


Fig. 1 and 2 — Static Tensile and Tensile Impact Bars

The value of preheating the steel and of stress relieving the weldments has been proven by past research on difficultly weldable steels. However, these operations are time consuming, expensive and sometimes impractical. The object of the study now being reported was to obtain the highest quality of arc weld possible without the aid of preheat or stress relief.

As far as the carbon content is concerned, hardenability increases with increasing carbon content to a maximum at the eutectoid composition. Consequently, any procedure that would successfully weld eutectoid rail steel should also be successful on any other rail steel composition, provided the manganese and other chemical elements remained about the same. Reference to the composition limits quoted above will indicate the probable variations to be expected. For this reason, the lots of rail steel chosen for arc welding tests were practically of eutectoid composition.

The welds were tested statically and under dynamic loading. Bar specimens for static tensile testing (Fig. 1) were prepared transverse to the weld with the joint at the mid-section of the throat of the specimen. Specimens were machined and carefully finished to avoid overheating the surface during the finishing operation.

Table I — Rail Steel Used for Welding

Steel	Composition			Tensile Strength	Elongation in 2 In.	Tension Impact
	C	Mn	S			
A	0.76	0.79	0.028	135,000 psi.	16.0%	151 ft-lb.
B	0.73	0.68	0.027	123,000	21.0	144
D	0.74	0.55	0.024	124,000	12.5	132
E	0.84	0.56	0.050	148,000	16.0	153
N	0.87	0.67	0.037	146,000	14.0	...
S	0.74	0.68	0.030	125,800	17.0	152

ples, wherein the beads were machined flush with the plate surface. A 1½-in. strip, with the bead remnant along the center line, was bent in a standard jig so the maximum fiber stress was parallel to the bead direction and on the machined bead surface. In these tests, bead ductilities improved with increase in heat input rate up to 11,000 joules per in. of bead. For higher heat inputs there seemed to be a slight reduction in the maximum angle of bend; this was attributed to increase in grain size in the heat-affected zone.

These preliminary tests indicated that a satisfactory heat input rate for arc welding this steel was 11,000 joules per in. of bead on ⅛-in. material. Figured back to the unit area of metal conducting heat away from a continuous bead, this value is $11{,}000 \times 4 = 44{,}000$ joules per sq.in.

Although this value is an optimum heat input for welds in this steel, the effects of higher heats are less serious than those of allowing the heat to drop below this optimum. Heat inputs for the welded joints later to be tested in tension and impact were based on the above optimum heat of 44,000 joules per sq.in. It was frequently necessary to exceed this rate with some weld-rods and in some types of joints to insure proper penetration and bead contour.

Test welds were made by hand, because equipment was not available for weaving the passes with automatic control. Also it was believed that the hand welds, while admittedly having poorer experimental control than the automatic welds, had a certain advantage from the practical standpoint because they illustrated what it was possible to do with manual control. Our welder had had comparatively little arc welding experience and it is probable that better results can be obtained in production.

Table II — Butt Welds in ⅜-In. Rail Steel Flats

Weld	Electrode	Weld Design	Average Heat, Joules Per In.				
			Pass 1	Pass 2	Pass 3	Pass 4	Pass 5
E7	2	A	59,000	62,000	61,500	60,000	
E8	2	A	49,000	61,500	66,500	47,000	
E9	2	A	52,000	73,000	70,500	66,500	
E11	1	A	38,500	52,500	59,500	52,500	
S18	2	A	36,200	31,000	33,400	40,500	38,500
S19	2	A	37,200	31,300	32,200	29,700	34,700
A1	1	B	50,000	55,500	58,000		
A8	3	B	55,500	50,000	60,500		
D4	1	B	51,000	58,500	55,000		
D9	3	B	57,000	56,500	62,000		
S5	1	B	54,500	58,500	57,000		
S15	3	B	57,500	55,000	57,500		

Table III — Static Tests on Welds

Weld	Base Metal Tensile Strength	Weld	
		Tensile Strength	Elongation in 2 In.
E11	148,000	68,000	3.5%
A1	135,000	93,500	5.0
D4	124,000	90,000	6.5
E7	148,000	111,500	3.5
E9	148,000	109,000	3.0
S18	125,800	111,500	7.5
S19	125,800	97,250	6.0
A8	135,000	116,000	4.0
D9	124,000	115,000	5.0

Table IV — Dynamic Tests on Welds

Weld	Tension Impact Value	
	Base Steel	Weld
S5-1	152 ft-lb.	58 ft-lb.
S5-2	152	44
E8-1	153	62
E8-2	153	37
S15-1	152	85
S15-2	152	10

Data on Test Welds

Data on the test welds are shown in Table II. The letters in the weld number refer to the lot of rail steel used. The electrode numbers refer as follows: No. 1 is an electrode of A.W.S. class E-7011 type and was used with alternating current. No. 2 is an electrode of A.W.S. class E-10010 type and was used with direct current, reverse polarity. No. 3 is a high carbon (1%) electrode used with direct current, straight polarity. All electrodes were $\frac{5}{32}$ in. nominal diameter.

All welds were single V, butt joints with back-up plate. Design A was a 60° V with ⅛-in. root; design B was a 40° V with ¼-in. root. Rail steel pieces were 5x3½x⅜ in., butt welded to form pieces 10 in. long. The pieces were clamped between chill plates overlapping to within 2½ in. of the weld on each side; the back-up plates were tack welded in place. Each pass was woven the entire width of the groove and all passes proceeded in the same direction across the joint. Interpass temperature was maintained at from 400 to 500° F. Test bars were then cut from the mid-section of the welds, rejecting the edges. The time for each pass was determined by a stop watch. Heat input was calculated from time, current and voltage. The optimum heat input for these joints should have been 33,000 joules per in. for each pass. The actual heat was slightly under in a few cases, but — as shown in Table II, above — usually far in excess of this optimum.

Results of the physical tests on bars from these welds are given in Tables III and IV.

Ductility is best in the five-pass welds S18 and S19. This seems to confirm the indication of the bead-bend tests, namely that some ductility is lost by a higher heat input rate per inch of pass. However, in these multiple-pass welds, the degree of grain refinement due to successive heat passes is also a factor to be considered.

Occasional micro-cracks are noted in the weld metal deposited during the first pass, observable only on etching a cross-section. It is significant that when they occur they always are *in the deposit* instead of in the heat-affected zone of the rail steel. There is no apparent correlation between the ductility of the weldments and the occurrence of these micro-cracks. They are attributed to the relative weakness of the deposit metal as compared to the rail steel and not to any brittleness in the rail steel caused directly by the welding heat.

The relative weakness of the deposit metal as compared to the base metal limits the overall ductility to be expected from the weldments. Most of the elongation under tensile test occurs within the weld metal itself. Due to this fact, the average ductility in a 2-in. gage length is much less than it would be if the weld metal and parent metal were of equal strength. It is believed that if an electrode were available which could lay a deposit that matched both the strength and the hardenability of the rail steel, the resulting weld would show a much higher figure for elongation. Numerous electrode types and makes were tried but none was found which completely met this dual requirement.

Conclusion — Welders generally believe that high carbon steels cannot be welded successfully without preheat and stress relief. The results herein reported seem to warrant modification of this general belief. Rail steel, and similar high carbon steels of the section weights discussed in this paper, can be welded with high heat input rates and with a good degree of success without the precaution of preheat and stress relief. The static and dynamic strength and the ductility of such weldments are quite satisfactory for many applications.

Fig. 3, 4 and 5 — Etched Structure at 100 Diameters at Edge of Heat-Affected Zone. Heat input: 5000 joules per in. for Fig. 3 at top, 7500 for Fig. 4 (middle) and 11,000 joules per in. for Fig. 5 at bottom. No martensite (white areas) exists under weld beads made with high heat inputs

CRITICAL POINTS

By the Editor

TO EDWARD G. BUDD'S manufactory in Philadelphia, birthplace of the all-steel automobile body in the halcyon days, now largely occupied with war and such projectiles as bazookas, rockets and large caliber high explosive shell. Also, since Budd is the leading proponent of strong, hard, cold-worked stainless steel sheet, spot welded into an engineering structure, talked with Colonel RAGSDALE about the expected post-war boom in passenger cars for railroads. Since only 6% of the present cars are less than ten years old — that is to say, modern — and about half of them are more than 25 years old — that is to say, antique — and also since railway executives generally hold that faster, more comfortable trains will be tomorrow's standard, it should follow that many light weight cars will be built of stainless steel as soon as conditions permit. Existing high speed trains of this construction, diesel powered, have amply proven their reliability, economy, attractiveness, and money-making ability. JOSEPH WINLOCK, chief metallurgist, also expounded his belief that cold-rolled stainless (18-8) is material par excellence for welding — especially by Budd's "shot welding" method, which, as all know, utilizes three or four cycles of alternating current to generate just enough heat at the faying surfaces of a lap joint to fuse a small nugget, so small that its boundaries do not reach the outside of the sheet.

Stainless steel sheet for easy welding

Stainless has high strength and great durability — good for the designer and owner. It has high electrical resistance, so it welds with relatively low currents and with correspondingly small and simple electrical arrangements. The sheet surfaces are free of hampering oxide, so no preliminary cleaning is necessary. There is no tendency for the electrodes to foul. Finally, and most important, the weld nuggets are "annealed" — that is, consist of softened austenitic steel — amply ductile to relieve the irregularities and internal strains left behind in any row of spot welds made under shop conditions. (We did not dwell upon the fact that the strong, springy metal is difficult to work, requiring its own machinery and techniques, nor that it takes clever engineering to dispose the expensive metal in such shapes and assemblies as to produce a light yet strong structure — both serious drawbacks, methinks, since the line of least resistance is into easy, unthinking things.) . . . . WINLOCK showed me some resistance welds made in a pile of six sheets of various thicknesses, ¾ in. in all. Some six "shots" of current are necessary, separated by enough time for the heat at top and bottom surfaces to dissipate through the electrodes. Finally a narrow "nugget" reaches from top to bottom, through all the sheets. When cut, polished, and lightly etched this nugget is a miniature ingot, with columnar crystals extending from its edges clear to its center, and yet, but an instant earlier, the crystals were microscopic in size. Imagine the speed of atomic rearrangement when such a heavily cold-worked material can convert itself into a casting by recrystallizing into columnar crystals ⅛ in. long in a third of a second, the entire duration of the six shots of current!

PONDERING further on this important matter of ductility for safe welded structures, and thought that "ductility" is really not the right word, for what is meant is the ability to move enough at localities — often microscopic in extent — so stresses can be relieved at places where they may become dangerous, *before* they become dangerous. The right word would also imply the property of stopping a crack as soon as it starts. "Crackless plasticity" has been used; let us assume it describes those desirable properties for weldments. Ability to be welded without destroying crackless plasticity, or, better,

Crackless plasticity

ability to perform as a tough metal after having been welded, would then be a qualitative definition for weldability of metal. And by this means we come to consider the matter of cracked ships and exploded gas containers....Proponents of the welding art have always argued that welding of itself and the welded joints are not solely responsible for failed structures, pointing to the fact that failures (cracks) often have origins distant from the weld, and the course of the crack has so often avoided the metal in the weld or the heat-affected metal alongside. Recently it has been stylish to ascribe failures in "rigid" welded frames — and bridges and penstocks fail as well as ships and containers — to locked-up internal stresses, or multi-directional stresses. This takes the discussion out of the field of elementary thinking, and puzzles the old-line engineer. He also asks why one ship cracks while 99 of the same design and construction do not, and the locked-up stress hypothesis has no answer. Many — including some vociferous politicians — blame the steel, yet without exception it has passed carefully drawn engineering specifications; ship plate, for instance, must be able to bend cold 180° around a small pin without sign of cracking. It is, of course, possible that the required tests are not searching enough, that they do not measure crackless plasticity (in the sense we have agreed to use the term). A suspicion that this may indeed be the nub of the question arises from the statement of ROBERT KINKEAD, a respected consulting engineer in welding, that his business always picks up in cold weather, for then there are so many failures to diagnose. If structures are safe in the summertime and fail in the winter, then *temperature* certainly has something to do with it. Temperature doesn't change the quality of the design or the welding, but does change the temperature differentials in exposed structures and therefore the imposed stresses, and also changes certain properties of the steel — measurably....It is worthy of thought and experimentation as to whether some property of the base metal may not be, after all, the most important factor in safe welded structures. It is well known, of course, that plain carbon steels which at room temperature have a good Charpy or Izod value — that is, absorb much energy to break a notched specimen — drop to a low Charpy or Izod value at lower temperatures. This transition occurs before the steel gets very cold, unfortunately sometimes no colder than +20° F.; in other steels (all but indistinguishable in composition) the transition from notch-tough to notch-brittle is at considerably lower temperature, frequently as low as —40° F. Some careful notched-bar tests on samples taken from cracked plates or shapes — tests at the temperature at which failure occurred — should be revealing. If they do give some correlated answers, then the manufacturers will have to find out how to make steels in all thicknesses and shapes, cheaply and regularly, with lots and lots of crackless plasticity. It is predictable that these steels will also be very "weldable".

Wanted: tough steels, tough even when cold

HEAD A LITTLE AWHIRL after listening to KENT VAN HORN, ASM's president, tell the Cleveland Chapter about the numerous aluminum alloys that have given this soft metal its manifold utilities. Methinks the words "tailor-made alloys" should be applied to them rather than to steels, for we have specialization carried to a high degree when an aluminum alloy that is strong and heat conductive (and therefore good for cylinder heads for air cooled engines) is modified further so as to reduce its expansion coefficient so it will make a better piston, more closely fitting the cylinder walls. Perhaps, though, the complexity is more apparent than real; VAN HORN's presentation left the impression of a large number of commercial alloys nearly all of them lacking names, having only hard-to-remember numbers on their dog-tags; had he attempted in a single talk to describe the copper alloys, cast and wrought, a similar feeling would have been retained by his hearers of a versatile material with amazing capabilities, whose multitudinous uses in numerous forms are capable of even further expansion as we learn more about its inherent excellencies.....However, there must be a limit, a ceiling, and it is impossible to resist speculation as to where it will be found. We know, for example, that C-67 or C-68 is the maximum hardness we can get in steel. What is a reasonable expectation of maximum useful strength in aluminum alloys? Metallurgists certainly have come a long way from the 13,000 psi. tensile strength and 35% elongation of commercial aluminum sheet to the 68,000 tensile and 19% elongation of the heat treated alloy known as 24S-T, the main reliance of our aircraft builders since 1932. A better gage of improvement is in yield strength, which was 5000 but is now 46,000! Later combinations of straining and artificial aging have increased the yield strength of 24S to 66,000 psi., but elongation has been sacrificed — down to 6% — and fabrication problems so multiplied that the strengthened material

Metallurgical versatility

Is there a ceiling?

has found but limited use.....The most recent advance in aluminum alloys of high strength was presented to the ASM convention in Cleveland last October by EDGAR DIX, of Alcoa's laboratories, in the form of alloy 75S-T. (Reynolds Metals Co. is also promoting a high strength alloy called R301.) Alclad 75S-T has a tensile strength of 76,000 psi., yield strength of 66,000 psi. and 11% elongation (typical properties for alclad sheet — extrusions are considerably stronger). Since this represents the culmination of 15 years of work on laboratory and commercial scale, one wonders if there is much more, really, *in* aluminum..... DIX, VAN HORN and their associates would probably reply, very understandably, "Don't sell our metal short!"

TO EAST ST. LOUIS and a stimulating day at the tightly built plant of Aluminum Ore Co. where, prior to 1938, all the ore for our pre-war aluminum was purified, and all the artificial cryolite and aluminum fluoride produced, necessary for cell feed. In the seven-fold war expansion, three more ore refining plants using the old Bayer process have been built, but are now all equipped to use the new combination process for recovering alumina formerly lost in the "red mud" tailings.....A plant using the Bayer process is an ore concentrator in the best sense, and has the ball mills, settling tanks, and filters of a western slime flotation mill, but instead of jigs, tables and flotation units one sees a maze of digesters, heat transfer units, and more pumps, pipes and tanks of assorted sizes than in a petroleum refinery, all to handle and transport expeditiously and economically thousands of tons daily of solutions and slurries. Truly, a chemical engineer's heaven!.....As is well known, the bauxite ore, in the Bayer process, is ground fine, digested with lime and sodium carbonate and the aluminum hydrate taken into solution, the insoluble filtered, and the aluminum hydrate crystallized out, washed, and calcined to Al_2O_3. Very neat! Unfortunately every pound of silica in the ore combines with a pound of alumina and another pound of sodium carbonate and stays in the red mud tailings, thus lowering the recovery and wasting reagents. Hence the demand for low silica bauxites — below 5% SiO_2 was the pre-war requirement. But unfortunately there is comparatively little such ore in the United States and in 1942 submarines in the Caribbean all but stopped its importation from Dutch Guiana. In fact, as the war went on, bauxites averaging 13% SiO_2 were treated, with corresponding loss in plant efficiency and manpower.....The emergency brought forth a process for retreatment of the enormous pile of red mud, invented by RALPH W. BROWN of Aluminum Co. of America's research staff. This lime-sinter or combination process (first described in *Metal Progress* for August 1942) uses long kilns to sinter a mixture of red mud tailings with lime and soda ash, thus breaking down and converting the insoluble sodium-aluminum silicate into compounds that yield 70 to 80% of the alumina when returned to the Bayer plant and leached there in process liquors. What this does to recoveries may be appraised by the following figures: Bayer process recovery on high grade bauxite (less than 5% SiO_2) is 90 to 95%. However on 15% SiO_2 bauxite it gets only 75%. The new combination process (at worst) then recovers 70% of the Al_2O_3 in the tailings. Over-all recovery becomes 92.5% — about as good as the old process on the highest grade ore. (The figure just quoted represents about the lowest expectancy. More representative would be: Bayer process gets 80% of the Al_2O_3 out of a bauxite containing 10% SiO_2; the lime-sinter combination process recovers 80% of the remaining 20%; over-all recovery is 96% — extremely good!).....What this does to our reserves of aluminum ore may be imagined. Facing the possibility that no bauxite could be imported, mining operations in Arkansas were expanded under W.P.B. urgings, so that in August of 1943 ore was mined at the rate of 8,000,000 tons a year, 20 times the rate of 1939. Meager reserves of good American ore have now been reduced to 5 million tons (7% silica or less); some 15 million tons are in sight of perfectly usable ore (15% silica or less) for the combined process. As the grade becomes lower, the available tonnage increases by geometrical progression. Really low-grade aluminum ore, such as high grade clay, probably never need be used, not only because high grade clay has far more valuable uses for refractories, porcelains and ceramics, but because there is probably a century's supply of low silica bauxite (3 to 5% SiO_2) in Dutch Guiana.....An indication that the Aluminum Co. of America intends to find out what combination of processes and modifications of unit operations will give best results for the least cost is the construction of a million-dollar research laboratory at East St. Louis — "pilot plant" they call it — really a complete ore treatment plant on a small scale, capable of studying reactions in a way that would be called "commercial" in most chemical processes.

Bayer process for refining aluminum ore

Lime-sinter adjunct

Alumina from clay

WELCOMED to the metallurgical research laboratories of Westinghouse in East Pittsburgh by HOWARD SCOTT and his associates and was again amazed at the enormous amount of information accumulated there in the last 20 years about complex alloy systems based on chromium, nickel, cobalt and molybdenum, frequently with additions of iron, titanium and tungsten — but always, as SCOTT says, "uncontaminated with carbon, which lets us out of a lot of trouble". Even after a promising alloy is uncovered by systematic study, it is an almost endless job to find the optimum composition and heat treatment, and determine its various physical, chemical and electronic properties at room and elevated temperatures. If you don't believe it, turn to P. H. BRACE's description of "K-42-B" in *Metal Progress* for March 1942; it's an alloy of Ni:Co:Cr:Fe (45:25:20:10) with a couple of parts of titanium added for precipitation hardening effect, and it creeps less than 1% as much at 50,000 psi. and 1000° F. as the best of the 27:22 Cr:Ni heat-resisting alloys. Even then (1942) BRACE stated that it was worthy of attention for operations at temperatures up to 1500° F. Thus is research forehanded..... Improvements have since been made in all stages of preparation for use; the later alloys are machinable and forgeable, non-scaling and strong at higher and higher temperatures, and having the property of hardening — either by precipitation or by "cold work" — at a rate that just about balances the creep effect. Because good creep and fatigue resistance are so important in high temperature alloys, methods for speeding up these snail-like tests are much used at East Pittsburgh such as stress-rupture tests for strength, and resonant systems capable of imposing ten million fatigue stresses a day. Appraisal of high temperature strength by stress-rupture in relatively short time is logical, also, since these alloys would have relatively short working life in current applications....."Practical" work on these alloys starts with the melt shop; to be produced commercially an alloy should be melted under slag rather than in a closed system or vacuum; next a workable ingot practice is necessary so the casting will be sound and free from slag or other dirt. It is fortunate that the Westinghouse studies have always paid a great deal of attention to the properties of the coarse-grained cast structure, since this is favorable for creep resistance. When comparing alloy to alloy it is also necessary to give the cast billet a standardized reduction by rolling, prior to recrystallization or other heat treatment.....SCOTT and his associates are now groping for light on the fundamentals, such as the mechanism of fracture, so they can better appraise the mountain of data already accumulated, and direct future studies along more likely paths. It seems reasonable to suppose that the reaction of metal to stress is related in some way to the proportion of crystalline grain material to the pseudo-amorphous grain-boundary material, since the mechanism of fracture through these two so-different states should be very different. But before one can even test this hypothesis it is necessary to know how to estimate the total *area* of grain boundaries in unit *volume* of material. WILLIAM JOHNSON has worked out the solution of this problem, given an ordinary photomicrograph large enough to show the boundaries of 100 grains. Some fairly encouraging relationships have already emerged from an analysis of many stress-strain curves (taken at constant rate of strain) when actual stresses are figured on the area of the reduced section. These relationships appear to hold through a wide variety of alloys and testing temperatures.

Systematic studies of high temperature alloys

Speed the testing

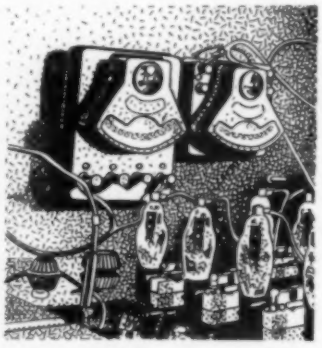
Estimates of grain-boundary areas

ABSORBED some of FRANK TATNALL's enthusiasm for the magical SR-4 strain gage, literally no bigger or heavier than a postage stamp, accurate to one part per million, costing only 50¢, and so fast in response that it can register strains in armor plate when hit by a bullet. It is simplicity itself, being merely a flat grid made of 5 in. of 1-mil copper-nickel wire, glued to a piece of cigarette paper. (Since they are used by the hundred thousand, perhaps here's another reason for the cigarette shortage.) For action, the gage is cemented to the metal at any desired point; it measures either tension or compression, for the cement is stronger and stiffer than the wire, thus preventing it from buckling. Electrical connections to a Wheatstone bridge and galvanometer measure its change in resistance with change in length (and corresponding change in diameter); speed is achieved by using alternating currents and electron tubes which amplify the infinitesimal changes within the strain gage into currents strong enough to register on oscillographs or to operate indicators, recorders, or even controls.....Aircraft wings and fuselages have been plastered with them, inside and out, and their changes in flight radioed to ground and there recorded — not slow changes, but flutter!

Magical strain gages

TATNALL says that these gages have at last enabled aeronautical engineers to *design* on a 100% basis, neither too strong nor too weak. Original design starts with many assumptions and guesses, and a complex structure is built as best they may. Strain gages are cemented to this at hundreds of doubtful points, the loads put on and elastic (even plastic) reactions studied step by step, always strengthening parts before they develop signs of crippling weakness and lightening other parts that are shown to be over-weight. This information enables a refined design to be made; this is checked similarly under load and re-studied under impact. Finally the prototype takes the air and its reactions at hundreds of regions are recorded by radio.....One doesn't need to be an aeronautical engineer to use SR-4's in his business. Take an engine case: Cover it with "stresscoat" — brittle paint that cracks when the metal underneath is stressed; overload the part slightly to find the danger spots; put strain gages on and around these places; study their reactions at operating loads and speeds; refine the design accordingly.....Or you might do this: If you have only a 100-ton press and need to do a 200-ton job, find out what part of your press is working closest to its safe limit, put a strain gage there, and let it tell you when to stop overloading and j-u-s-t avoid trouble. Maybe your 100-ton press is really strong enough for 300 tons!

Design 100% and work to very limits



IMPRESSED, on each visit to Baldwin's locomotive factory near Philadelphia, with the feeling that nothing much is doing; there is hardly a sign of traffic within the spacious yards, yet there are 20,000 men working within the tremendous buildings scattered all over the place. It's not so surprising, however, if you know that each shop is so well contained that it is a complete stock yard, factory and warehouse all of itself, under one roof. The boiler shop, for instance, finishes boilers for three locomotives every day, yet that takes only one switch engine one trip between boiler and erecting shops. Gargantuan positioners take a tender for welding entire, so the noisy riveting on boilers seems an anachronism, but PAUL IRWIN, Baldwin's engineer of tests, my guide, said the Interstate Commerce Commission's safety committee is still sharply restrictive of welded joints, believing locomotive road service to impose more strains than does sea-going service aboard warships, for example, where welded drums with much higher pressures are the rule. Maybe some day FRANK TATNALL will get around to selling the I.C.C. some of his SR-4 strain gages and the idea that they might as well stop guessing about the matter and find out the facts!.... Railway equipment — "hell for stout" — doubtless is different from most other machinery. IRWIN says one of the nightmares is "maintenance" men who use polished siderods for anvils when they have to do a little rough blacksmithing.....Observed a neat machine for flame hardening the curved slot in reversing links. Both torch and quenching head are guided by a cam and a program control that delays forward motion until the end area is properly heated, switches on the quenching water at the correct time, and then stops everything at proper sequence at the far end. The job is done in minutes where it used to take hours to carburize and quench, and there is no warpage outside of the narrow grinding limits.....Much interested in some high speed movies (1500 frames per sec.) of a scale model of a new truck for high speed cars, as it traveled over bad rail joints at 100 miles per hr. (The "track" was a pair of flywheels of diameter properly proportionate to the wheels on the truck; the bad rail joint was a toolsteel knife-edge set into the rim.) A grid of wire was mounted in front of this model; the camera lens had a deep focus so each frame on the film shows truck, reference grid, and time clock. Better than a stroboscope, for the frames can be projected on a screen, one by one, time-position curves can be plotted for any point on the truck or its supported car, and these vibratory lines studied and analyzed at leisure....Such high speed photography reminds me of some rather unbelievable work by EDWARD THILO at Frankford Arsenal. His problem was to photograph a 20-mm. high explosive shell as it passed through armor plate, exploding enroute and flaring out a "tulip" in the plate five times the shell's diameter. That wouldn't be so much of a trick in photography and timing for the boys at the Arsenal, except that the explosion would demolish the camera as well. To protect the camera, a ¼-in. armor shield was necessary; to photograph through the shield, X-rays replaced visible light.

Three locomotives every day

High speed — really *high* speed

GAS FURNACES FOR INTERRUPTED QUENCHING

By A. H. Koch
Surface Combustion Corp.
Toledo, Ohio

THE VERY THOROUGH analysis that has recently been given the quenching phase of the heat treatment of steel has created a decided interest in what is referred to as "interrupted quenching". Basically, this consists of quenching into a medium maintained at a temperature considerably higher than the 75° to 150° F. range of the usual oil, water, brine or air quench. Interrupted quenching, combined with the recent advances in the knowledge about the relationships between alloying and hardenability, is a primary contribution to the art of improving physical properties, preventing internal strains from quenching, and reducing distortion.

A complete discussion of the metallurgical principles involved in the various types of quenches was given by Arnold P. Seasholtz in *Metal Progress* for October 1944, so this matter need not be discussed at length. It is worthy of emphasis, however, that the steel must be heated to and held at a temperature above its upper critical long enough to transform and diffuse it into an austenitic structure as homogeneous as practically possible. For proper and uniform response to any rapid cooling *the austenite must be uniform.* The time required for the transformation of austenite to start, and to be completed, is dependent upon the analysis of the steel and the temperature at which the steel is held, provided of course this temperature is below the critical. These transformation times at constant sub-critical temperatures are plotted in the well-known S-curves, the best collection of which may be found in the "Atlas" issued by U. S. Steel Corp. Research Laboratory. A reproduction of the S-curve for X-4130 is shown in full lines in Fig. 1, page 922.

Where the upper part of the S-curve (representing the beginning of transformation) approaches and reaches the minimum time period is commonly called the "nose". In Fig. 1 this is located between 900 and 1100° F. The "critical cooling rate" for quenching this steel is that rate at which the time spent in cooling from the upper critical to the temperature at the nose of the S-curve is less than the minimum time at the nose. To attain the maximum hardness the actual rate of cooling in the quench must be greater than the critical rate of cooling.

It must be remembered that the S-curves for isothermal transformation do not map the transformations which occur during continuous cooling. The S-curves of Davenport and Bain are plotted on the basis that the steel sample is cooled *instantly* to a given temperature (say 1100° F. on Fig. 1) and held constant at that temperature while the transformation proceeds. The intersection of the curves with the horizontal ordinate (at 1100°) then records the fact that *under these conditions* austenite does not start to transform for 1½ sec. (Point *x*). Ferrite then starts precipitating and it takes about 35 sec. for all the excess ferrite to appear (Point *y*). Then only does some carbide come out of the remaining austenite, and fine pearlite ("primary troostite") starts to form. When temperature remains steady at 1100° F. the entire body of the steel is transformed after a time represented by Point *z*, or 33 min.

In normal quenching in the usual mediums

the temperature of the steel is dropped from above the upper critical to slightly above room temperature as rapidly as possible Under these conditions the curves representing times and temperatures for the beginning of transformation are displaced to the right and downward from their location in the S-curve for isothermal transformation. This fact was recognized by Davenport and Bain and their associates, and by C. A. Liedholm of Curtiss-Wright Corp., who developed a ready method of plotting the transformation curves for continuous cooling by a study of the Jominy hardenability test bar. His results for X-4130 are shown in Fig. 1 in dotted lines (see *Metal Progress* for January and November, 1944). Fortunately, for thoroughly hardening pieces of considerable size, *on continuous cooling*, the transformation at the "nose" of the curve (900 to 1100° F.) does not occur in this hardenable alloy steel short of 8 sec. Once below that temperature the austenite is fairly stable—that is, takes considerable time to transform, persisting for 13 sec., say, at 700° F. as shown by Point *u* — down to what is known as Ar″ or the "martensite point", Ms, when martensite transforms very rapidly on further cooling.

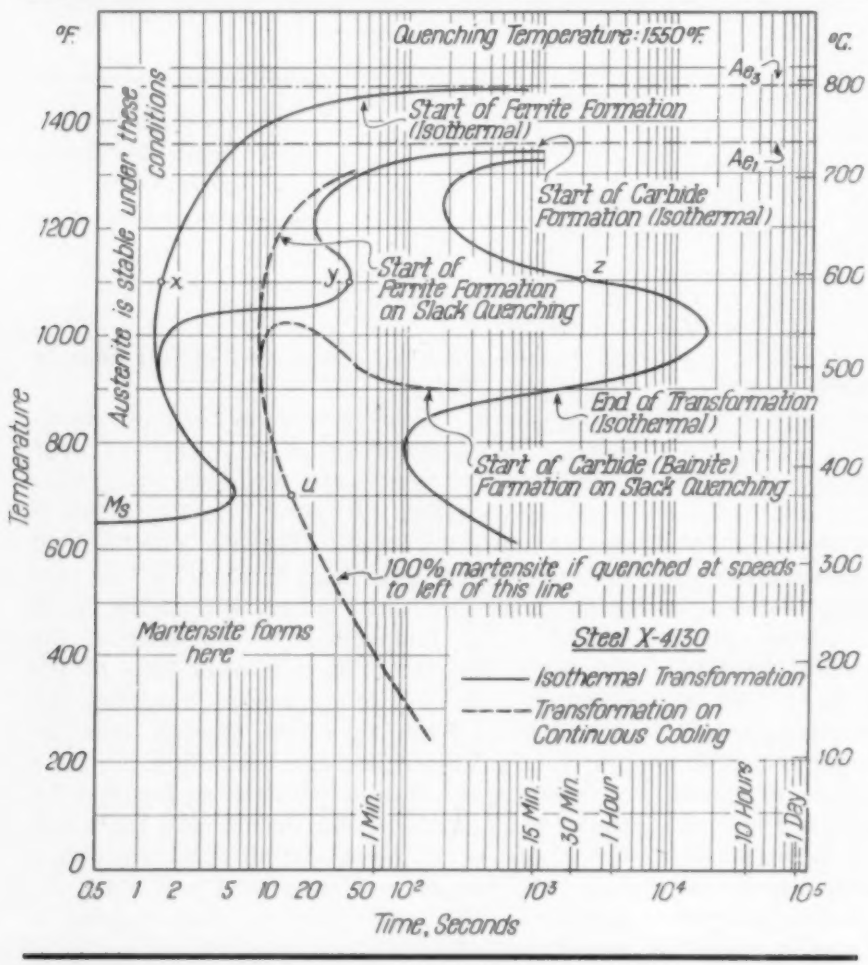


Fig. 1 — Transformation Curves for S.A.E. X-4130. Full lines are for isothermal transformation (from U.S.Steel Corp.'s "Atlas of Isothermal Transformation Diagrams"). Dotted lines are for transformation during continuous cooling as in oil or water quenches (from C. A. Liedholm's data sheet in Metal Progress, November 1944)

Normal Oil Quench — During a rapid, direct quench the outer surface of the steel cools faster than the core. The outer shell reaches the Ms point before the inside of the steel cools that far and forms a hard, brittle martensitic case enclosing a core which still contains a considerable volume of austenite. When this austenite transforms later in the cooling into martensite, it increases in volume, creating large tension stresses in the outer case. This is one of the causes of distortion and, if of sufficient magnitude, also will cause surface cracks and possibly breakage during quenching. Such small surface cracks and even micro cracks within the steel greatly reduce the ductility of the steel, relieved only partially by subsequent tempering — which cannot, of course, heal the cracks.

To avoid this troublesome state of affairs three methods of interrupted quenching are being used. Briefly they are as follows:

Austempering — Quenching rapidly into a molten bath at some appropriate temperature below the nose of the S-curve and above the Ms point. This retains the austenite untransformed for a certain period and then transformation proceeds as indicated by following across the S-curve for the steel being quenched at the bath temperature. When transformation is complete the steel is cooled in air. The result is usually "bainite", a mixture of ferrite and carbide of still unknown nature, of medium hardness but unusual ductility and toughness. However, for many types of steels

the holding time in the bath at desirable temperatures is extremely long, which makes the process impractical commercially. Tests also indicate that the high quenching temperature required for some steels makes it impossible to stay within the critical cooling rate; this "crossing the nose of the S-curve" produces a mixed microstructure — usually containing primary ferrite — a structure having low physical properties. For these reasons austempering is limited to steel parts of small section and of certain analyses.

Isothermal Quench — To avoid some of the above defects the steel is withdrawn from the austempering bath in reasonably short time and immediately heated to a higher temperature to increase considerably the speed of transformation. After complete transformation the steel is cooled in air. This method can be used for steel parts of relatively heavy section and for higher physical properties than is possible with austempering.

Martempering consists of quenching in a molten salt bath maintained at a temperature slightly above where martensite begins to form (Ms point), and held there until the temperature has equalized throughout the entire cross section, but not long enough for isothermal transformation to commence. It is then removed from the bath and cooled slowly in air; austenite thereupon transforms into martensite at a more or less uniform rate throughout the thickness of the steel, thus avoiding excessive residual strains. If needed, the steel, after air cooling can be subjected to the usual tempering treatment.

Martempering can be utilized for hardening heavier sections than can be hardened by any other known method of interrupted quenching, and is especially valuable for steels which are normally known as "shallow hardening".

Quenching Media — Oil cannot be considered for interrupted quenching because the quench temperatures are in excess of or in too close proximity to their flash point. Molten salt has been found to have sufficiently low viscosity, and the ability to absorb heat at a rapid enough rate to serve for this purpose. It has, therefore, become the most commonly selected medium. Agitation of the salt bath is not necessary except for relatively high rates of production in small baths, or if parts are to be quenched in dense masses.

"Controlled gas atmosphere quenching", which consists of rapid cooling in a prepared gaseous atmosphere vigorously circulated in large volume, may in the future be used for interrupted quenching of thin and medium sections. Development and research to date indicate that this method is feasible and offers many potential advantages.

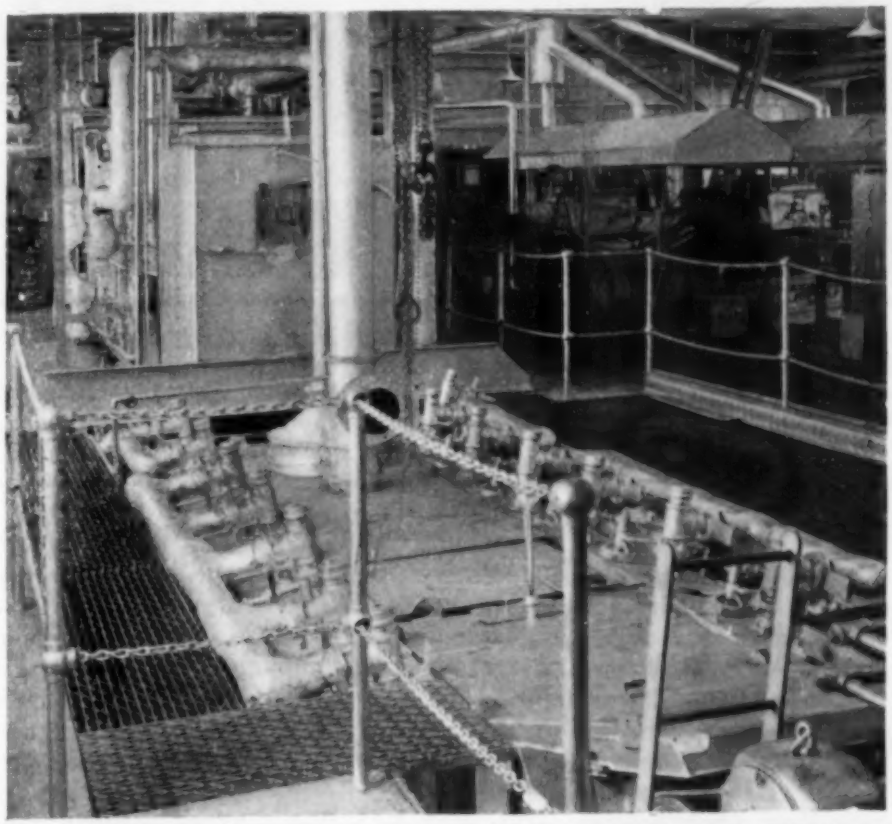


Fig. 2—Rectangular Pot Quench Furnace, Heated by Two Rows of Immersion Burners, and Equipped With Continuous Conveyor (Discharge Apron at Lower Right). In the background is the hardening furnace; it has a controlled atmosphere and is also heated with radiant tubes

Hardening Furnace — If steel, when quenched in a salt bath, is covered with scale or other foreign matter, a residue will collect in the bottom

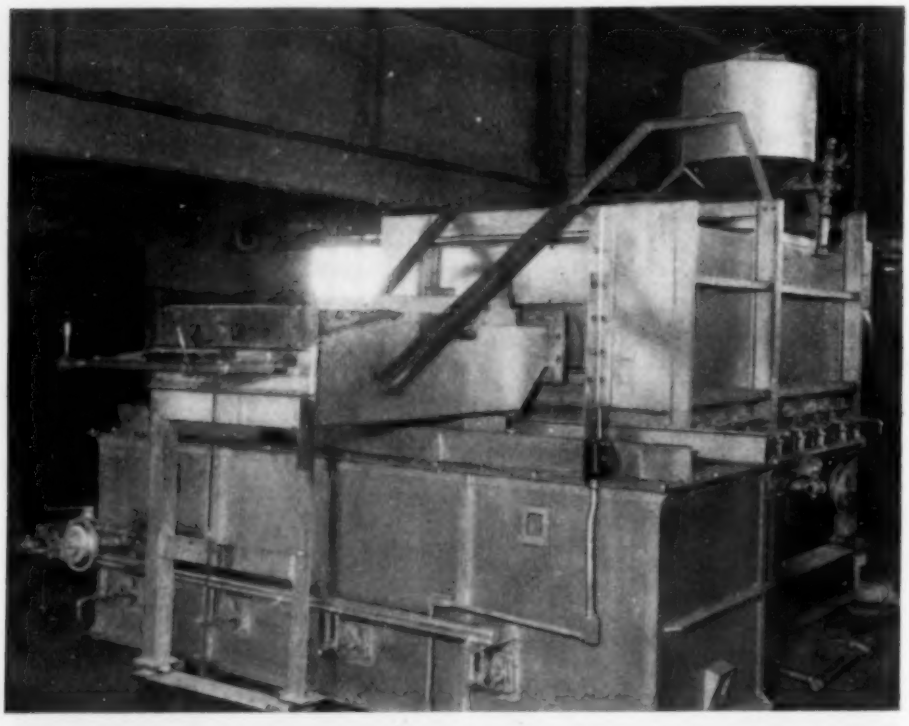


Fig. 3 — An Externally Fired, Pot Quench Furnace, Receiving Work Through an Elongation of the Muffle of a Hardening Furnace Set Immediately to the Rear. The furnace has a pusher for moving the work through the heating cycle; note the charcoal container above the furnace binding, for preparing correct protective atmosphere (Char-Mo gas)

of the salt bath which will require periodic removal. Furthermore, a sufficient concentration of iron oxide will react with and cause the salt to deteriorate. Therefore, the hardening furnace must be of a type that will prevent scaling during the heating before quenching.

If the steel parts are heated in a lead bath they will have to be held down in the bath by a fixture, or in a covered, wire mesh container to keep them from floating. This may be rather difficult to do if many differently shaped parts must be heat treated. There will also be a carry-over of lead, clinging to the hot steel parts, which will collect in the bottom of the quench bath and will have to be removed periodically.

If a salt bath is used for heating the steel to above the upper critical temperature, there will be a carry-over of this salt with higher melting point into the quench bath of lower melting point. This will gradually increase the viscosity and reduce the quenching properties of quench salt, or form a sludge in the bottom of the quenching pot; either way the quench bath will require close attention and periodic rejuvenation.

How frequently such difficulties will arise will depend upon the quantity of the carry-over. Parts with smooth, straight surfaces, not densely loaded, will have a relatively small carry-over, while parts with holes or recesses or parts closely packed will have a high carry-over.

Salt bath or lead hardening furnaces are, therefore, to be recommended only for moderate production rates, provided the form of the parts and the method of handling is such that carry-over will be limited.

For other applications and for positive insurance against quench bath difficulties, a controlled atmosphere type of heating furnace is the best choice. Parts should be well cleaned before being charged into the furnace. The composition of the atmosphere to be used in the furnace will depend upon the analysis of the steels to be processed. (See articles by Floyd Harris in *Metal Progress* for January and April 1945.) One which will prevent scaling, decarburization or rapid carburization should be selected. A furnace for this type operation is shown, from exit end, in Fig. 3.

Salt Quench Furnace — Pot furnaces for salt quenching must be maintained at a constant temperature, irrespective of the rate at which they are operated, and therefore capable of meeting three distinctly different operating conditions:

1. Salt must be melted and the bath brought to temperature when originally charged or after a shut-down. This stage requires a high rate of heat input.

2. The salt bath must be maintained at temperature during stand-by periods. This requires a low rate of heat input, merely sufficient to make up for normal radiation.

3. During quenching the heat introduced into the quench salt by the hot steel will quickly increase its temperature. Therefore, a means of rapid heat removal must be provided.

To meet these conditions a very successful design of pot quench furnace has gas fired, suction type, immersion burners, with heating tubes inside the pot directly in contact with the salt. At the inlet end of each heating tube is an

inspirator and piloted gas burner. At the outlet end is an eductor or exhaust fan to draw the products of combustion from the tube, and to provide sufficient additional suction to draw a large portion of the air for combustion into the inlet end.

About 40% of the required air is mixed with the gas in the inspirator and the remainder is drawn into the tube around the burner as secondary air. As the gas and air continue through the tube in parallel streams they gradually diffuse into each other and burn. This provides a uniform heat output along the entire length of the tube, irrespective of the number of bends required to conform to the pot's contour. Such a furnace — rectangular in shape, installed in a pit below floor level, and with W-shaped burner tubes extending down both sides and across the bottom — is shown in Fig. 2, page 923.

During quenching periods, when heat must be rapidly extracted from the salt, the gas supply to the heating tubes can be manually or automatically shut off. The eductors (or an exhaust fan) continue to operate, and usually will draw sufficient cold air through the tubes to remove the required amount of heat. If necessary, provision can be made to force additional air through the cooling tubes to increase the quenching capacity of the bath.

For relatively low production rates a small draw furnace of the pot type, with low-pressure automatic proportioning gas burners, may be used. (With this firing system air under pressure inspirates the proper quantity of gas for combustion.) During quenching periods the gas supply may be shut off and the air normally required for combustion allowed to circulate through the furnace setting. A furnace of this type is shown in Fig. 3.

Without air cooling, the only way heat can be dissipated is by normal radiation. The amount of heat so radiated will depend upon the dimensions of the pot, the ratio of pot depth to its diameter or width, and the insulation provided in the furnace setting. An analysis of 15 different sizes of externally fired pot quench furnaces is shown in Table I. The marked increase in quenching rate obtainable when cooling air is allowed to flow through the burner equipment is clearly indicated by this analysis.

Table I — Quenching Rates (Lb. per Hr.) for Small Pot Furnaces

Pot Dimensions; In.				Pot Volume; Cu. Ft.	Furnace Lining Thickness		Quenching Rate From 1600 to 500° F.		Ratio of Increase
Dia.	Width	Length	Depth		Insulating Firebrick	Insulation	No Air Cooling	Air Cooled	
12	—	—	12	0.66	4½ in.	—	4.6	12.5	2.72
12	—	—	18	1.00	4½	—	6.0	17.8	2.97
18	—	—	12	1.30	4½	—	7.4	23.5	3.18
—	18	18	12	2.25	4½	—	9.5	28.5	3.00
18	—	—	18	2.20	4½	—	9.2	31.5	3.43
24	—	—	18	4.00	4½	2½ in.	14.0	36.0	2.57
—	22	22	10	2.80	4½	2½	14.8	38.5	2.60
—	15	29	12	3.00	4½	2½	14.4	42.5	2.96
—	18	18	18	3.37	4½	—	11.1	42.5	3.83
—	18	27	12	3.37	4½	—	13.8	42.5	3.08
—	22	22	18	5.00	4½	2½	16.5	48.5	2.94
—	18	27	18	5.10	4½	—	16.3	55.0	3.38
—	22	36	12	5.50	4½	2½	21.5	63.5	2.95
—	36	36	15	11.30	4½	4½	27.1	81.0	2.98
—	20	40	24	11.00	4½	2½	26.4	100.00	3.79

Theoretically, it appears possible to increase the rate of quenching by installing additional cooling coils in the salt bath. Two of the earlier installations equipped with suction immersion burners also included such additional cooling coils. However, in actual operation they have never been required.

Air rather than water is recommended as the cooling medium. If a water coil should leak or break when immersed in molten salt, the resulting shower of scalding salt is hard to contemplate.

After the parts are taken from the quench and cooled they are washed in hot water to remove the salt which clings to them. An ingenious idea adopted by one metallurgist who has a continuous operating unit, is to drain the waste water from the washing machine back into the top of the salt pot. Here the water evaporates rapidly, steaming off without any harmful effect. This not only cools the bath, but also returns the salt otherwise lost down the drain.

Temperature can be easily controlled, automatically, either in the immersion burner or externally fired pot quench furnaces. A gas valve controlled by the temperature of the salt bath is all that is necessary. When the desired temperature is attained the gas supply is shut off and the air, normally used for combustion, continues to flow through the burner equipment. When the temperature falls below the desired point the gas is again turned on.

Walter Edwin Jominy

SAUVEUR ACHIEVEMENT MEDALIST AMERICAN SOCIETY FOR METALS, 194

WALTER EDWIN JOMINY

ONE OF THE LAWS of life is that a biography appearing in a professional journal must stress that the subject is "a Prince of a Fellow", and must endow him with some if not all of the moral attributes of a Boy Scout.* As a result, those readers who know the hero intimately are often astounded to learn that his is a truly Christ-like character, even though their experience had indicated otherwise. However, in the case of Walter Edwin Jominy — chief metallurgist of the Chrysler Corp.'s Dodge Chicago Plant and the subject of *this* brief biography — most of his acquaintances would agree without mental reservation that the first six attributes of a good Scout were certainly applicable, and they would give him the benefit of the doubt on the rest.

But even a man who has not known Walter Jominy long enough to determine whether he is Brave or Obedient or Reverent can't avoid being impressed with his tact and his complete lack of "side". His tact is not studied but inherent, and is of such quality as to allow him to become outspokenly forthright if he has to, yet not violate the feelings of those who are his momentary opponents. He has been in the metallurgical end of the motor industry for almost 30 years and is currently in the front rank of his profession. But, like most top-notchers, he doesn't exude the air of authority. With his round, beaming face and his little black mustache he looks more than anything like a successful French chef in street clothes! (Actually, he is not irritable enough to be a chef.)

*According to the Boy Scouts' Official Handbook, a Scout is: "Trustworthy, Loyal, Faithful; Friendly, Courteous, Kind; Obedient, Cheerful, Thrifty; Brave, Clean, and Reverent."

Detroit is the home town of Walter Jominy; he was born there on June 16th, 1893, and went to the public schools there. (His family came originally from Switzerland and his great-grandfather was a general in the armies of Napoleon. After Napoleon met his Waterloo, great-grandpa went to Russia and continued his career in the army of the Czar.)

When he graduated from Detroit's Central High School he entered the University of Michigan where he studied chemical engineering. He says that he "half worked his way through college", probably a typical understatement. The fact is that, as a Freshman, he supported himself by drying dishes and gradually improved himself so that, as a Senior, he was in business for himself as a pants presser. He received his Bachelor's degree in 1915, and a Master's degree in Chemical Engineering the following year. During this last year at Michigan he had the good fortune to be laboratory assistant to the famous Professor Edward deMille Campbell who, as everyone knows, was blind as a result of a laboratory explosion. As Professor Campbell's eyes and hands, Jominy conducted his laboratory experimentation, read the newspapers and scientific journals to him, and absorbed much from him in return — as is, indeed, true of everyone who came in contact with that great man.

Not long after his graduation from Michigan he was taken into the U. S. Bureau of Aircraft Production where he devoted his talents to the famous Liberty engine of World War I as senior inspector, stationed in various plants such as Packard and Ford. All told, he has spent five years at the Studebaker Corp., two years at Packard, one year with the Liberty engine, seven years as research metallurgist at the University

of Michigan, three years with the A. O. Smith Co. in Milwaukee, seven years in the General Motors Research Laboratories in Detroit as research metallurgist, and four years with the Chrysler Corp., where he is now chief metallurgist of Dodge Chicago Plant making the famous Wright Whirlwind motors.

Thus the cycle is complete, starting and ending with aircraft engines.

The people in General Motors Research remember Jominy fondly for what appeared at first to be a delightful eccentricity. There are a lot of earnest young Doctors of this and that working in the G.M. labs, and Jominy found it impossible to remember all of their names and still get his work done. Nothing is so sacred to a man as his name, correctly spelled and pronounced, and Jominy realized that many a good man might come to feel both slighted and discouraged if his associates couldn't remember his name; his work must then be unappreciated. In a triumph of tact, then, Jominy discarded baptismal names entirely and nicknamed most of his co-laborers. Under this system it was not unusual for an *extremely* dignified young man to find himself referred to as "Biddletwit" or an overly meticulous one as "Hair-Breadth Harry".

In 1918, Jominy married Mildred Hood Ryrie, a St. Louis girl, and they have a daughter and a son — the latter now in the Navy. In normal times the Jominy family used to spend their vacations at Mullet Lake, where Jominy likes to fish — presumably for mullet. At home he has always kept a flower garden where he tends his roses — however this is not a scientific pursuit and he has not yet attempted to produce a new hybrid to be known as the Jominy Rose. He is also fond of music, although he doesn't play anything. Probably his greatest admiration, aside from the hardenability band, is for nobly written prose, and so he is especially fond of re-reading Thackeray's "Vanity Fair".

Once, as a sideline to metallurgy, he dabbled in the banking business. He became vice-president of a bank to please a doctor friend who had started the institution and wanted Jominy's steadying influence. "It was but a small bank," Jominy is careful to say when he tells about it, lest some Radical accuse him of being one of the Morgan crowd. Anyway, he was able to sell his interest just ahead of the depression — not that he foresaw it coming, but because he was moving to Milwaukee to work for A. O. Smith.

Jominy, who is a member of the Sigma Xi Honorary Society, the American Foundrymen's Association, and the American Society for Metals, is the author of some 25 technical papers, most of them bearing on the treatment and behavior of steels used in the automobile manufacturing industry, and largely relating to developments of general interest, such as his work on hardenability testing.

At a time when it is commonly said that the war is proving to be a great boon to industrial research, Jominy maintains that the reverse is true. Industrial research, he thinks, has actually been retarded in recent years. There are several reasons for this, he says, one of which is that "the incentive for industrial research is lessened by the ease with which standard articles may be sold at any reasonable price, so that the immediate urge to develop new articles, or lower the price of standard articles, is not very great. Industrial research is usually in either of these directions."

"In addition," he says, "the great drain on manpower with good technical training has caused many men to be moved from the research field into production work. It is true that the increased technical requirements of war materials have forced industry to become more 'technical minded', and, as a result, new equipment and better process control have been introduced. The greatest contrast is seen in the smaller shops, but even in the larger organizations a greater degree of technical control is being used than before the war. Much of this control, however, is the result of pre-war research and development, was already in effect in the leading industries before the war started, and is only now being more widely distributed."

At the last annual dinner of the ⊜ in Cleveland during the National Metal Congress, Walter Jominy was awarded the Sauveur Achievement Medal for his work in developing hardenability testing. In accepting the medal, he made a little speech that lasted about one minute and thirty seconds, and what he said was perhaps as characteristic of himself as any other single example that might be picked. He began by stating, in no uncertain words, that the success of his work was due to the efforts of several people who must share in the credit, particularly Al Boegehold, head of the metallurgical department of General Motors Research Laboratories, under whom he worked, and he ended by saying simply and quietly that his gratification in receiving the medal was made complete by the fact that it had been given to him in the presence of so many of his old friends. All of the thousand persons present knew that this was not merely a graceful speech.

EDWARD C. MCDOWELL, JR.

CORRESPONDENCE AND FOREIGN LETTERS

Extra High Carbon During Long Cementations

TORQUAY, DEVONSHIRE, ENGLAND

To the Readers of METAL PROGRESS:

Your editor has asked me if the cementite in "converted bar" (for crucible melting or shear steel) is, exceptionally, very high on its surface.* If by "high" you mean well in excess of 2%, I should say "No".

But I must not mislead or say what I do not know. The converted bar was graded by a skillful man. He might throw out a bar because it was "aired" (decarburized) on the surface, but I never heard of a throw-out because the carbon was exceptionally high. The converting process was entirely a rule-of-thumb procedure and in my early days (1883) no one, except Sorby and later Arnold, made micro-examinations. The fractured parts of the bars were sorted into groups according to their average carbon content; that is, the sorting put the broken bar into groups which would produce the indicated carbon when melted in the prevailing clay crucibles. No one talked about "carbon"; the sortings were made into numbered groups.

There are still converting furnaces in Sheffield and the broken bar is still being used, exceptionally, for making toolsteel. The temperature of the last converting furnace I saw in operation was controlled pyrometrically, and I was told that bars could be carburized to a predicted degree.

While my acquaintance with converted bar does not include the occurrence of surface carbons greatly exceeding what A_{cm} requires, I do not deny the possibility of such occurrences. I know that armor plate kept in a carburizing furnace for 10 to 14 days at 1000° C. (1825° F.) might have cementite bunched together at its surface beyond expectation. I thought the bunching together of cementite beyond A_{cm} limits might be due to considerable variations in furnace temperatures, which, on the drop, caused cementite to fall out of solution and, on the rise, took new cementite into solution more readily from the carburizing gases.

I also once had something to do with deeply carburized rectangular sections, and often suspected the corners contained more carbon than A_{cm} permitted. The "freckled corners", as they are called, were unwelcome and were moderated by choosing particular carburizers or by reheating to cause diffusion.

The massive cementite formed in the surface of the armor plate was undesirable, and might be machined off. To avoid this a mild steel sheet, 3 to 5 mm. (0.16 in.) thick, was laid between the thick plate and the covering solid carburizer. The thin sheet "sieved out", as it were, the unwanted cementite; I regret having never examined one of the cementite sieves.

HARRY BREARLEY

*Readers may remember a letter in the correspondence pages of *Metal Progress* last July from Harry W. McQuaid calling attention to the fact that many articles commercially carburized at about 1700° F. have a carbon content at their very surface far higher than the amount indicated by A_{cm} for 1700° on the equilibrium diagram, A_{cm} being the maximum found by Floyd Harris in his notable series of articles on gas carburizing. Mr. McQuaid suggested that unavoidable temperature fluctuation was a likely cause of excess surface carbides. The Editor, mindful of the fact that converted bar has been made for generations by heating wrought iron in charcoal for 7 to 10 days at about 2000° F. (boxed, in large coal fired furnaces with human eye pyrometers and all sorts of chances for temperature variations), wrote Harry Brearley asking whether unduly high carbon contents were often the result. (A_{cm} for 2000° F. is about 1.6% carbon, and "Grade 5", known as "steel-through heats", the highest carbon grade of single converted steel, contains an average of about 1.25% carbon, ranging from about 1.5 at the surface to 1.1% at the center.) Mr. Brearley, although best known to Americans for his invention of the stainless cutlery steels, has been associated with the fine steel industry since his early youth, and has written a fascinating book on crucible steel making entitled "Steel Makers".

Rapid Hand Polishing of Micro Specimens

FRANKFORD, PHILADELPHIA, PA.

To the Readers of METAL PROGRESS:

The Ordnance Laboratory at Frankford Arsenal has been using essentially the same methods as described by Anton L. Schaeffler in *Metal Progress* last August. We distinguish between "research samples" and "routine examination"; "research" samples are repolished on the final wheel and re-etched two or three times. Experience confirms all the statements made with respect to speed of the method, retention of inclusions, and general quality of surface.

Frankford Arsenal has two comments which may be of general interest:

1. The "gamal" abrasive has been found to polish most efficiently only if the acidity of the suspension is maintained in the very narrow range of pH 7.4 to 7.8. If the suspension is more alkaline (pH over 8.0), the polishing time required for satisfactory results rises to an amazing degree, while, if the solution is on the acid side (pH less han 7.0), relief effects and etching are frequently noted. This Arsenal adds a mild alkali (triethanolamine) to the dilute suspension until the pH indicated above is obtained.

2. This Arsenal prefers to use ceresin which is harder than parowax as a base for the wax lap. A suspension of 600X alundum in soap solution has been used on the wax lap rather than levigated alumina.

C. H. GREENALL
Lt. Col., Army Service Forces,
Ordnance Dept.

Tool Life and Power Required for Finishing Cuts

ANN ARBOR, MICH.

To the Readers of METAL PROGRESS:

We have received some comment on the "Metal Cutting Nomograph" published as a data sheet in April 1944 *Metal Progress*, to the effect that it gives no information on tool life or power required for light finishing cuts. The reply must be that the data sheet is only an approximate solution to a very complex problem; many compromises have to be reached. One of the compromises in our nomograph was to limit the feed to above 0.005 in. per cut. We did this intentionally because our tests have shown that when smaller feeds are used considerable variation in test results will be found. The heat treatment, the method of manufacture, as well as the chemical composition of the material cut have very different effects on tool life than when larger feeds are used. With very light feeds, we have found that the abrasive qualities of the materials being cut exert themselves to a far greater extent than with larger feeds. The method of sharpening the tools and the tool materials themselves also are far more critical when using light cuts. We have found that the order of superiority of the different materials being cut may change when light feeds are used as compared to heavy feeds. The Brinell hardness of the material cut may be used as a fair indication of machinability when using heavy feeds, but when light feeds below 0.005 in. are used, hardness is not a reliable indicator because, as remarked above, the abrasive qualities of the material begin to exert their influence. Again, depth of cut is of smaller importance in screw machines or in turret lathes working on bar stock of small diameter.

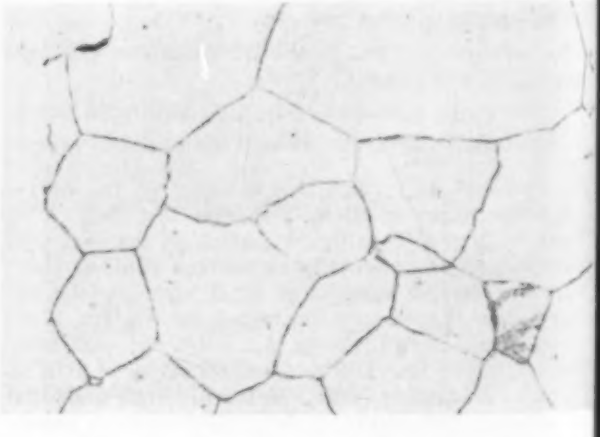
W. W. GILBERT
Metal Process Dept.
University of Michigan

Russians, Russians Everywhere

MELBOURNE, AUSTRALIA

To the Readers of METAL PROGRESS:

A tube of unstabilized 18-8 from an ammonia heat exchanger showed considerable grain boundary carbides, and down in the corner what appears to be a bearded character in an astrakhan



cap. It seems likely that this man is a Russian, and if Germany cracks up like this stainless tube, there won't be much left.

B. R. MARTIN
Metallurgist
Munitions Supply Laboratories

Copper-Manganese-Nickel

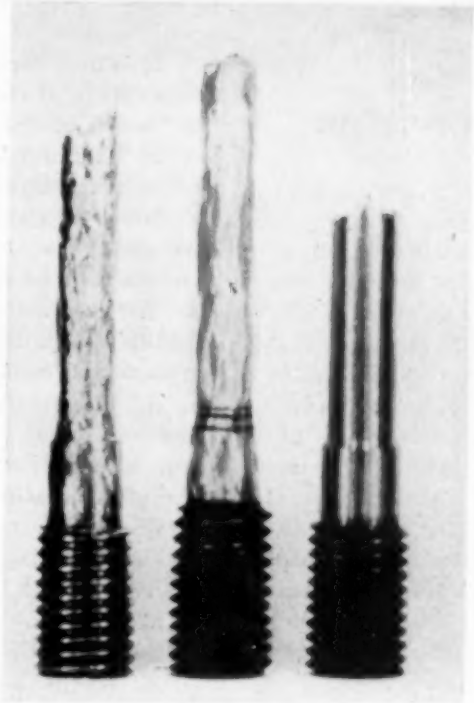
ROLLA, MO.

To the Readers of METAL PROGRESS:

Carbon-free alloys of manganese are being studied with considerable interest, and some unexpected properties have resulted from various

aging treatments — that is, unpredicted by the accepted theory of precipitation hardening. However, the purpose of the present note is to call attention to the extraordinary power of work hardening of the 60:20:20 Cu-Mn-Ni alloy, as cast and after solution treatment.

The left-hand bar in the group is the original cast alloy. Its hardness is Rockwell B-36, yield 20,800 psi., ultimate strength 47,700 psi., and it had 40% elongation in 2 in. A companion bar (shown at middle) was heated for 18 hr. at 1200° F. to put soluble constituents in solution and then water quenched to hold them there. Its properties were B-42, 19,000 psi., 46,250 psi. and 50%.



Three Tensile Test Bars of 60:20:20 Cu-Mn-Ni Alloy, Respectively As Cast, Solution Treated and Aged

Note the large spread in each of these conditions between yield and ultimate (more than 100%), a true figure for work hardening ability, since the elongation was distributed generally from end to end of the test bar.

A third bar given the above solution treatment and then aged 24 hr. at 800° F. was hardened to a rather unusual extent (B-103). Its yield was 70,000 and ultimate 75,800 psi., and the test piece is characteristic of this small plastic range.

CHARLES Y. CLAYTON
Prof. of Metallurgy & Ore Dressing
Missouri School of Mines

Advice Requested on Spectrographic Standards

WASHINGTON, D. C.

To the Readers of METAL PROGRESS:

National Bureau of Standards is preparing to expand its list of standards for the spectrographic analysis of irons and steels beyond the provisional list now available (see *Metal Progress* for March 1944, p. 502), and desires the advice of industrial and scientific users. Of the spectrographic standards now in use, the 400 series (rods $\frac{7}{32}$-in. diameter by 4-in. long) apparently satisfies the needs of most users and this size will be continued. The Bureau is doubtful, however, about electrodes for the point-to-plane excitation technique. The question is whether the present ½-in. diameter by 2-in. rods should be replaced by a larger disk, such as one of 2-in. diameter.

Two disadvantages of the ½-in. rod may be listed: *(a)* It needs an adaptor to be mounted in a holder such as the Petrey stand; *(b)* the limited area may possibly introduce variable excitation and analytical results — although the Bureau has found no significant difference in determinations made from ½-in. and 1½-in. flats of the same material.

While the 2-in. disk will doubtless be more convenient to use, *(a)* it requires a much larger quantity of metal, *(b)* it will be more costly to obtain homogeneous bars, *(c)* it will be wasteful if used for preparation of special shapes.

All spectrographers are therefore requested to advise the Spectroscopy Section of the National Bureau of Standards as to the reasons for their preference for the ½ or the 2-in. size (since both cannot be provided, in addition to the $\frac{7}{32}$ × 4-in. size which will be retained).

Furthermore, the Section would be glad to get a list of the complete requirements of each laboratory for iron and steel spectrographic standards *in order of importance* beginning with those of greater importance. Assume that no standards are available and list the requirements for *all* types, including cast iron, low carbon steel, high carbon steel, low alloy steel and high alloy steel, including stainless and toolsteel. Indicate the types of steels, the elements and the concentration ranges for which certification is desired.

LYMAN J. BRIGGS
Director
National Bureau of Standards

Incubation of Centers for Austenite Transformation

CHITENAY (LOIR-ET-CHER), FRANCE

To the Readers of METAL PROGRESS:

Students of isothermal transformation of austenite know that changes in alloy steel become apparent in the microstructure only after a stay of some time at the given temperature. This period of apparent inactivity is ordinarily called the "incubation time". It may be supposed, alternatively, that *(a)* the austenite does not change during this period of inertia, *(b)* some unobservable change or true incubation of unknown character is taking place, or *(c)* the normal transformation process starts as soon as the steel is cooled below Ar_2 but the initial rate of change is so small that its transformation products are not discoverable by the means now at our disposal.

Certain observations discredit the first of these hypotheses:

1. The quiescent period varies *in a definite manner* with the temperature of the hot bath into which the steel is quenched, for a given steel and given prior treatment.

2. Steels which *complete* their transformation slowly at a given temperature usually *start* their transformation slowly, and vice versa.

3. If the amount of transformed austenite in proportion to the total volume of the steel is plotted against time, one will find that the curve starts slowly, increases in slope to a maximum and then reverses its trend, approaching completion very slowly. These curves show no sign of discontinuity, and the American metallurgist Mehl was well advised, in his mathematical study of the kinetics of the phenomenon, to place the origin of his theoretical curve at zero time.

Some quantitative studies on the influence of different factors on the "incubation" time, made in collaboration with Henri Jolivet, have brought some new light on the nature of this phenomenon. In our work we adopted the convention (familiar to testing engineers in their 0.2% offset method of determining yield strength) of assuming the start of transformation as that time when 1% of the volume of the austenite has transformed. We find the following facts to be true, for the steels investigated, and for temperatures not too far below Ar_1:

1. The "incubation" time varies with the temperature of isothermal transformation (above the "nose" of the S-curve).

2. The minimum time of incubation (shown as the temperature of the nose of the S-curve) is at a temperature slightly lower than the temperature of the maximum rate of transformation. Another way of saying this is that the minimum time for complete transformation into ferrite and pearlite is usually slightly higher than the temperature for minimum incubation time.

3. The minimum time of incubation increases linearly with the austenizing temperature, although the temperature for minimum time of incubation remains constant. Thus, for a steel with a nose at 1100° F. on the S-curve marking the beginning of transformation, the incubation time is 20 sec. after the steel is austenized at 1475° F., 60 sec. after austenizing at 1650° F., and 100 sec. after austenizing at 1830° F.

4. If the austenizing heat is of relatively short duration, the S-curve representing beginning of transformation depends measurably on the prior heat treatment of the steel. For example, the curve for a steel previously annealed (furnace cooled) from 1550° F. will be to the left of the curve for the same steel previously normalized (air cooled) from 1650° F.

5. Incubation time is shortened if the steel is first quenched in a bath at some higher temperature and held there for a short time. For example, a high alloy steel whose minimum incubation time (6 min.) occurred at 1100° F. was austenized at 1650°, quenched to 1200° F. and held there 4.5 min., and then transferred to the second bath at 1100° F., whereupon transformation started almost immediately. In fact the entire transformation-rate curve was moved bodily parallel to itself by this pre-quench.

6. The shortening of the incubation time, noted immediately above, is proportional to the time at the upper quenching temperature (in proportion to the incubation time at that temperature).

Such experimental findings indicate that some "preparation" for visible transformation occurs during the incubation time, and this unknown mechanism is a definite function of time and temperature. As the incubation time is especially affected by temperature and time of austenization, being the longer for more uniform austenite, the preliminary invisible actions may

well be associated with the birth of nuclei of the excess constituent (cementite or ferrite, as the case may be). From these first centers, transformation progresses normally by growth and precipitation of cementite and ferrite; meanwhile new isolated nuclei are being germinated which will, in turn, create new centers of transformation.

ALBERT M. PORTEVIN
Consulting Metallurgist
Bessemer Medalist

Mathematical Demonstration of Hardenability Principle

TOLEDO, OHIO

To the Readers of METAL PROGRESS:

When the hardenability of a steel is computed from its composition, as by Grossmann's now well-known method, the operations are based on the proposition that the effect of several different alloying elements of equal hardening power is greater than the same total amount of any one of them. Practically all know that this is true, for the NE steels (the "triple alloy" steels) economize on alloys in exactly that way; a little chromium, nickel and molybdenum takes the place of a lot of chromium or nickel. Mathematically it can be proven:

To show that the effect on hardenability of several different alloying elements of equal hardening power is greater than the same total amount of any one of them:

Let A and B be two alloying elements for which the relationship between hardenability factor and alloy content is linear (which is true for most elements),

Let $H_{(a,x)}$ be the hardenability factor for x% of A, $H_{(b,y)}$ be the hardenability factor for y% of B, k_a and k_b be constants for A and B respectively, which represent their hardening powers.

$$\text{Then } H_{(a,x)} = 1 + k_a x$$
$$H_{(b,y)} = 1 + k_b y$$
$$H_{(a,x+y)} = 1 + k_a(x+y)$$
$$\text{and } H_{(a,x+b,y)} = (1 + k_a x)(1 + k_b y)$$
$$= 1 + k_a x + k_b y + k_a k_b xy$$

Now if $k_a = k_b$, as for elements of equal hardening power,

$$\text{then } H_{(a,x+b,y)} = 1 + k_a(x+y) + k_a^2 xy$$
$$\text{hence } H_{(a,x+b,y)} - H_{(a,x+y)} = k_a^2 xy$$
$$\text{Similarly, } H_{(a,x+b,y+c,z)} - H_{(a,x+y+z)} =$$
$$k_a^2(xy+yz+zx) + k_a^3 xyz$$

The difference is positive. Q.E.D.

ROGER F. MATHER
Chief Metallurgist
Willys-Overland Motors, Inc.

Free Oxygen Prevents Sulphur Attack From Hot Furnace Gases

SHEFFIELD, ENGLAND

To the Readers of METAL PROGRESS:

For a long time the presence of appreciable amounts of sulphur in furnace gases has been regarded as being likely to cause a notably increased attack on heat resisting steels exposed to their action, even at only moderately high temperatures. While opinion has probably differed to some extent as to whether this or that alloying element improved or reduced the resistance of the special steel under such conditions, there was almost general unanimity that high nickel was detrimental.

Probably this belief was based on the fact shown by J. F. Kayser in Britain in 1924 (*Transactions* of the Faraday Society) that sulphurous gases could form a eutectic of nickel and nickel sulphide on the alloy surface melting at about 1200° F. He experimented with alloys containing 60% nickel or more but it was assumed that a similar effect — though in less degree — would result with the austenitic chromium-nickel steels of considerably lower nickel content. It was, in fact, sometimes stated that such steels were unsuitable for exposure to products of combustion of fuels containing more than about 1% sulphur, and that one should then use either the straight chromium steels or similar steels containing suitable amounts of silicon or aluminum or both.

In some respects, however, these steels have less desirable properties than the austenitic chromium-nickel steels, and continued observation of the behavior of the latter with sulphur-containing furnace gases showed that they were not always as bad as they were painted, the key to the situation being the presence or absence of a sufficient amount of free oxygen in the sulphurous furnace gases. If these gases contained perhaps 3 or 4% oxygen a protective scale was formed, and even such a high nickel alloy as 25% chromium, 20% nickel could be used satisfactorily. On the other hand, incomplete combustion, resulting in the presence of unburnt fuel in the furnace gases instead of free oxygen, produced a scale containing sulphide; and even if there was not sufficient sulphide to produce a

fusible layer at the working temperature, its presence made the scale more permeable and hence less protective to the underlying metal than a normal oxide scale.

Nickel sulphide, of course, is not alone in forming an easily fusible eutectic; iron sulphide also forms one with iron which melts at about 1800° F. Less is known about chromium sulphide but Houdremont and Bandel reported from Germany in 1937 that steels containing even 32% chromium when tested in pure hydrogen sulphide (thus preventing completely the formation of an oxide scale) were resistant only up to about 1100° F., and that under such conditions there was little difference between the behavior of austenitic chromium-nickel alloys and the plain chromium steels of similar chromium contents. Evidently chromium sulphide is far more permeable and far less protective to the underlying metal than chromium oxide.

Confirmation of the deleterious effects of sulphide in the protective oxide film formed on various steels — heat resisting or otherwise — and of the beneficial effect of excess oxygen in preventing its formation, has recently been provided in a series of reports by the British metallurgist, A. Preece, and his collaborators at Leeds University. This is one result of an extensive investigation on scaling under the auspices of the Alloy Steel Research Committee of the Iron and Steel Institute. The work is still in hand and has been concerned up to the present in determining the effects of varying composition of furnace gases on the scaling of steels during heat treatment and forging operations, and hence has consisted of relatively short-time tests, often at temperatures above those at which the individual steels have any marked degree of resistance to scaling. However, the results show quite definitely the importance of free oxygen in sulphurous gases. An earlier report dealt with the effects produced at 1000° C.; the 1944 report carries the story up to 1150° C. (2100° F.).

Preece used a gas containing 80% nitrogen, 10% carbon dioxide and 10% water vapor, a composition which represents the gaseous products of complete combustion of most liquid or gaseous fuels in the correct amount of air. He studied in detail the rate of scaling of various types of steels after adding to this "neutral" gas, as he termed it, known amounts of oxygen or carbon monoxide (representing respectively excess air or incomplete combustion) in the presence or absence of sulphur dioxide.

The results showed that whereas the rate of scaling of steel containing up to about 18% chromium, with or without nickel, was approximately doubled by the addition of 4 or 5% oxygen to the "neutral" gas at 1825 to 2100° F., the addition of 0.2% sulphur dioxide (an amount such as would occur in the products of combustion of crude coke oven gas containing 600 gr. S per 100 cu.ft., or of producer gas made from coal containing 2 or 3% S) quadrupled the rate of attack. If, however, 4 or 5% oxygen was added *in addition* to the sulphur dioxide, the rate of scaling fell to that obtained when oxygen alone was added, and the scale produced contained little or no sulphide.

While it is agreed that these tests were carried out at temperatures considerably above those at which the individual steels would ordinarily be used as heat resisting steels, the results emphasize the role played by oxygen in preventing the formation of less protective scales containing sulphides.

J. H. G. MONYPENNY
Metallurgist
Brown Bayley's Steel Works, Ltd.

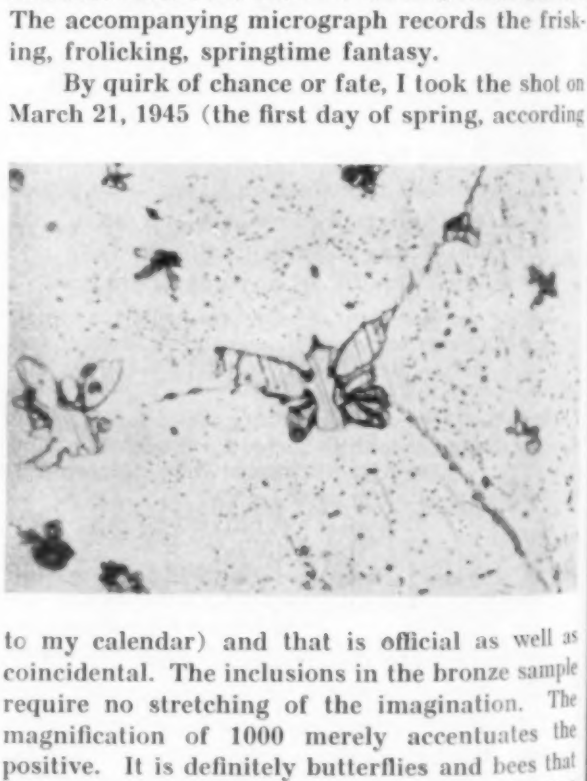
Fantasy in Bronze

DETROIT, MICH.

To the Readers of METAL PROGRESS:

Spring, spring, beautiful spring as butterflies and bees herald the season even in a research lab. The accompanying micrograph records the frisking, frolicking, springtime fantasy.

By quirk of chance or fate, I took the shot on March 21, 1945 (the first day of spring, according



to my calendar) and that is official as well as coincidental. The inclusions in the bronze sample require no stretching of the imagination. The magnification of 1000 merely accentuates the positive. It is definitely butterflies and bees that frisk so gay and carefree across the view, in what appears to be a snow storm.

J. F. COULTIER
Omega Research Laboratory, Inc.

QUENCHING OILS

By Blaine B. Wescott
and L. W. Vollmer
Gulf Research & Development Co.
Pittsburgh, Pa.

IN THE DECEMBER 1944 columns of "Bits and Pieces" in *Metal Progress*, page 1273, METALLURGICUS featured his own remarks and opinions on quenching oils. This was the last of a series of short notes on oil quenching equipment and techniques, the immediately preceding one being on page 713 of the October 1944 issue, and it discussed the problem of controlling the temperature of quenching oil. The following takes issue with some of his opinions because his publications perpetuate data and conclusions that are invalid, and present views that we are sure are not in accord with established facts and practices.

The writer who signs himself METALLURGICUS made frequent favorable reference to the paper entitled "An Evaluation of Quenching Oils" by SPRING, LANSDALE, and ALEXANDER (ASM *Transactions*, Vol. 33, 1944, p. 42), and portions of those discussions of the paper which served his brief. But thus far no mention has been made of the discussion of that same paper by the present writers, wherein it was pointed out that the cooling curve apparatus used by SPRING and his associates was inadequate — a fact that would have become evident if they had attempted to confirm their conclusions by simple hardening tests.

SPRING, LANSDALE, and ALEXANDER concluded "that but little if any difference existed in the capacities of the various oils to remove heat, and that those differences could be adjusted by control of the bath temperature". These conclusions were based solely on cooling curves registered for the center of a $\frac{13}{16}$-in. diameter specimen of 18% chromium, 8% nickel austenitic alloy, determined with the aid of a time-temperature recording apparatus utilizing a Leeds and Northrup mirror galvanometer. The usual Leeds and Northrup laboratory type of mirror galvanometer has a period of at least 2 sec., and therefore is totally incapable of correctly indicating the rapid changes in rates of cooling that occur in a $\frac{13}{16}$-in. specimen quenched in oil.

The similarity of the cooling curves for the dissimilar oils tested, the failure to indicate all three of the well known stages of the cooling cycle, and the unusually long time to cool from the quenching temperature to, for example, 750° F., compared with published data on oil quenches of similar metal sections (see H. J. FRENCH's book on Quenching of Steels, p. 60) all clearly indicate that the temperature measuring apparatus was far too sluggish for determining reliable cooling curves.

We have had an opportunity to test the "Oil A" of SPRING's experiments (a mixture of 100% animal oils). Our equipment — which we hope to describe fully in a future issue of *Metal Progress* — includes a temperature recorder which makes use of an oscillograph with a period of 0.0192 sec. This instrument has shown that a 27-gage thermocouple, quenched from 1500° F. in brine, cools to 750° F. at the rate of 14,300° F. per sec.

The cooling curve determined with this apparatus, for the 18-8 specimen and Oil A, used by SPRING, LANSDALE, and ALEXANDER, is compared in Fig. 1 with curves plotted from the data for Oil A listed in Table 2 of the paper in ASM *Transactions*. The cooling rates at 1300° F. reported in the paper (for Oil A at 130° F. bath temperature) were 32.4° F. and 29.6° F. per sec.; the curve obtained in Gulf's research laboratory with the fast apparatus shows the rate to be 80° F. per sec.!

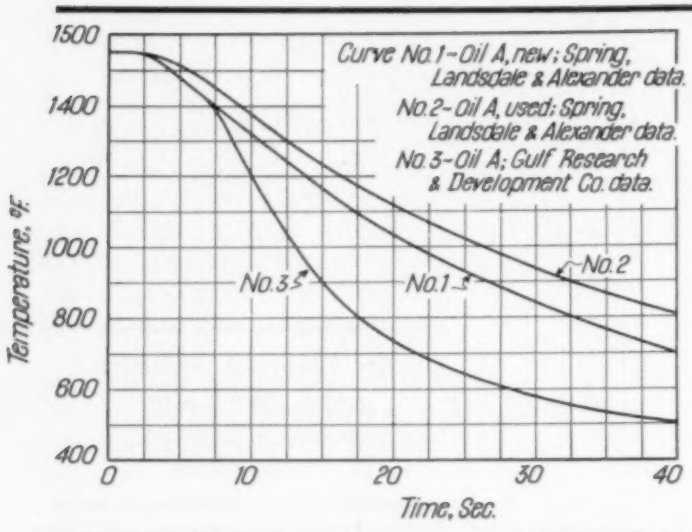


Fig. 1 — Cooling Curves for Center of $\frac{13}{16}$-In. Cylinder of 18-8 in a Certain Oil, no Agitation. Curve No. 1 and 2, as reported by Spring, Lansdale, and Alexander for new and used oil, respectively. Curve No. 3, as reported by Wescott and Vollmer

It is very evident that the curves in the paper are more nearly a representation of the dynamic or deflection characteristics of the long-period galvanometer, used by the experimenters, than a measure of the quenching capacities of the oils. Under these circumstances it is to be expected that all oils would appear to quench at nearly the same rates. Oils do *not* quench at nearly the same rates, as the curves in Fig. 2 show. The rate of cooling at 1300° F. in the slow, plain mineral oil (METALLURGICUS's "10¢-a-gal. oil") is only 32° F. per sec. (Curve No. 2); in the fast mineral oil it is 92° F. per sec. (Curve No. 1). Oil A mentioned above (Curve No. 3) is shown to be a fast oil, which accounts for its long continued use for much difficult hardening work; it is expensive, however, because of its high percentage of whale oil, and does not have the chemical or thermal stability of mineral quenching oils.

It has been shown repeatedly that — other than the initial improvement resulting from a decrease in viscosity — increasing the oil temperature within practicable limits has little or no influence on its quenching capacity. No amount of manipulation of bath temperature will equalize the hardening power of the slow and fast oils of Fig. 2.

A misinterpretation may be placed on METALLURGICUS's statement that a moderate change in degree of agitation is enough to wipe out the slight difference between the H value of 0.30 in oil without agitation (as reported by GROSSMANN) and FOCKE's maximum H value of 0.44 for an *unagitated* proprietary oil. Identical degrees of agitation will not equalize the quenching capacities of slow and fast oils, and violent agitation of the fast oil will provide a severity of quench that cannot be attained in a slow oil. It is obvious that agitation can provide equal H values only *within the range of a slow oil's quenching capacity.*

Space does not permit an extended report of case histories on fast oils, but the experience of LANGSTON and PEASE given in their discussion of the above mentioned paper in *Transactions* is not an isolated one. As long as the hardenability of steels remains highly variable and uniformity of the quench is desired, the use of a fast oil quench usually can be justified. A fast quenching oil is certainly not a *substitute* for good quenching technique and equipment, but rather a *supplement* to them.

As METALLURGICUS has stated, water in quenching oil seriously impairs quenching efficiency, but it is sheer waste to discard an otherwise usable oil because of water contamination — as he advises — regardless of its cost. Water can be removed from oil often by simply settling and decanting, and always by heating the oil to the boiling point of water. Also, filters are available which will remove not only water but also suspended scale and sludge, thereby improving the efficiency of oil coolers.

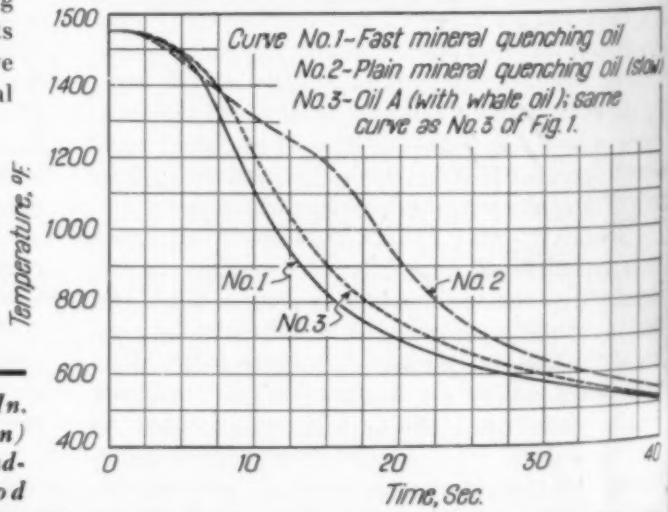
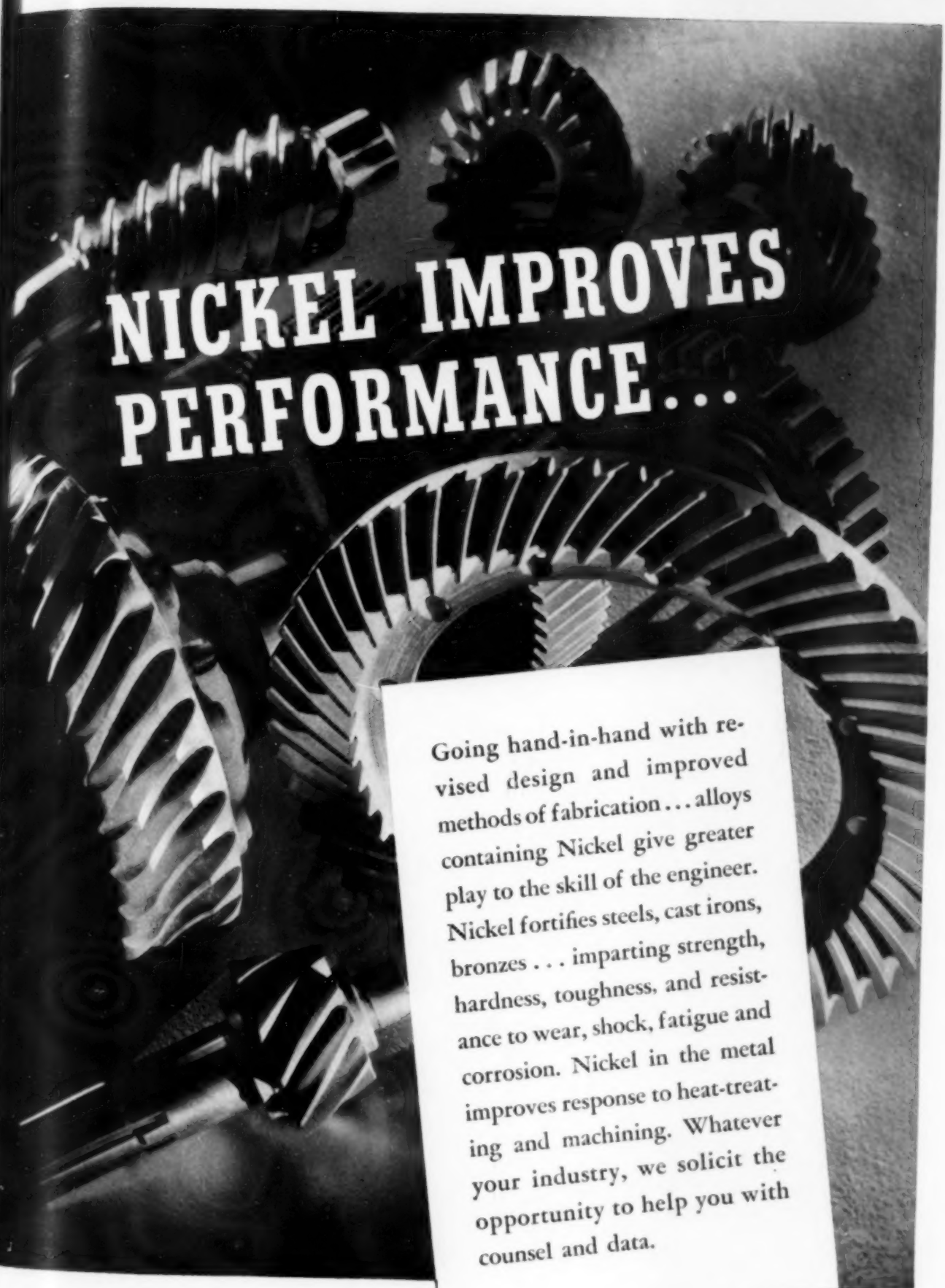


Fig. 2 — Cooling Curves for Center of $\frac{13}{16}$-In. Cylinder of 18-8 in Three Oils (no Agitation) Determined With Sensitive Apparatus Including Oscillograph With 0.0192-Sec. Period



Going hand-in-hand with revised design and improved methods of fabrication... alloys containing Nickel give greater play to the skill of the engineer. Nickel fortifies steels, cast irons, bronzes . . . imparting strength, hardness, toughness, and resistance to wear, shock, fatigue and corrosion. Nickel in the metal improves response to heat-treating and machining. Whatever your industry, we solicit the opportunity to help you with counsel and data.

THE INTERNATIONAL NICKEL COMPANY, INC. 67 WALL STREET NEW YORK 5, N. Y.

The A-B-C of Corrosion and Heat Resisting Steels

Prepared by the Research Division of Rustless Iron and Steel Corp. (April, 1945)

Group A (Martensitic) *Straight Chromium; Hardenable*	Group B (Ferritic) *Straight Chromium; Non-Hardenable*	Group C (Austenitic) *Chromium-Nickel; Non-Hardenable*
Type Numbers 501, 502, 403, 405, 410, 414, 416, 420, 420-F, 431, 440-A, 440-B, 440-C, 440-F.	430, 430-F, 442, 443, 446.	301, 302, 302-B, 303, 304, 308, 309, 310, 316, 321, 347.
Chemical Analysis Chromium 4.0 to 13.5% with up to 0.15% carbon, or chromium 12 to 18% with carbon 0.15 to 1.20%. Most types contain molybdenum, up to 0.60%. Some contain nickel up to 2.50%, or sulphur or selenium up to 0.40%. Magnetic.	Chromium over 14% to 27% with carbon not over 0.35% (generally 0.15% or less). One type contains up to 1.25% copper, one sulphur or selenium up to 0.40%. Magnetic.	Chromium 16% to 26% and nickel 6 to 22%. Carbon from about 0.05 to 0.25% (generally less than 0.15%). May contain molybdenum, columbium, titanium, sulphur or phosphorus and selenium. Non-magnetic.
Heat Treatment Harden by air or oil quenching to 350 to 620 Brinell, depending on carbon and chromium content. Can be drawn back to develop a wide range of mechanical properties. (Brinell 140 to 600.)	Do not harden significantly. Should be annealed by cooling rapidly from around 1500° F. for maximum toughness and good corrosion resistance. (Brinell 140 to 190.)	Do not harden by heat treatment, but can be hardened by cold work to high hardnesses. Should be annealed by cooling rapidly from 1800 to 2050° F. for maximum softness and corrosion resistance. (Brinell 140 to 185.)
Toughness High bend toughness and good notch toughness when properly heat treated. Generally lose some notch toughness when tempered from about 750 to 950° F., or when exposed to low temperatures.	High bend toughness, but notch toughness may be low depending on composition and heat treatment. Overheating lowers toughness; additions of nitrogen increase it. Notch brittleness disappears at elevated temperatures.	Unusually high toughness, unaffected by notches. Retains toughness down to liquid air temperatures. Exposure at 800 to 1600° F may impair toughness of non-stabilized types.
Structural Changes at High Temperatures Not subject to excessive grain growth. Grains may be refined by heat treatment. Mechanical properties not impaired by prolonged exposure to temperatures up to 1400° F., and structural changes negligible. Low chromium alloys contain molybdenum to prevent temper brittleness.	Types especially low in carbon and high in chromium are susceptible to marked grain growth when heated for long periods over 1650° F. Grain growth is reduced by nitrogen in alloy.	Austenitic alloys precipitate carbides at grain boundaries when heated to or cooled slowly through the range of 900 to 1650° F., becoming susceptible to intergranular corrosion and losing some toughness. This is controlled by very low carbon content, or by addition of titanium or columbium.
Strength at Elevated Temperatures Creep strength much superior to plain carbon steels up to 1200° F. Retain tensile properties to 750° F.	About same creep strength as Group A. Load carrying properties are low at temperatures over 1300° F.	Excellent creep strength up to 1200° F., which is further enhanced by molybdenum or columbium. Load carrying ability superior to Groups A and B.
Hot Working Qualities Readily forged, rolled or pierced from 2350 to 2000° F. High carbon types should be preheated and worked at lower temperatures, and low carbon and free-machining types worked at higher temperatures. Air hardening characteristics may call for slow cooling after forging.	Readily forged, rolled or pierced. Should not be overheated nor soaked long to avoid grain growth. Usual forging temperature around 2200° F. Finish around 1450° F. to refine grain. Alloys do not air harden significantly.	May be forged, rolled and in most cases pierced. Usual working temperature 2250° F., but free-machining type with sulphur may require higher temperatures. May be water quenched after working without harm.
Cold Working Qualities In annealed condition the cold metal can be easily drawn, rolled, upset, bent, formed, coined or deep drawn. High carbon and free-machining types are more difficult to work. Work-harden slowly, like carbon steels.	Well adapted to all types of cold work. The alloy with 17% chromium is especially suited for forming and deep drawing. Alloys work-harden slowly, like carbon steels.	Can be readily cold worked by drawing, rolling, upsetting, forming, deep drawing, etc. Work-harden rapidly, compared to carbon steels, and may be cold drawn or rolled to high tensile strengths.
Machinability Machining rates of regular types are about 50% of bessemer screw stock; free-machining types up to 80% (with high speed or carbide tools). Tools and work should be rigidly mounted. Suitable for automatic screw machining.	Machining rates and tooling practices similar to Group A. Suitable for automatic screw machining. Sulphur-base oils recommended for Groups A, B and C. Tools should be honed for maximum production.	Machining rates of regular types are about 40% of bessemer screw stock; free-machining types up to 75% (with high speed or carbide tools). Tapping, threading and drilling require reduced speeds.
Riveting Suitable for cold rivets. Hot riveting above about 1450° F. not recommended because of air hardening properties.	Suitable for cold rivets. Hot riveting requires care to avoid brittleness; rivets should be driven at 1425° F. into chamfered holes.	Excellent for cold and hot rivets. Hot rivets may be driven at any temperature over 1600° F.
Welding Properties May be welded with gas or electric arc, or by resistance. Preheating and post-annealing generally desirable to avoid cracking. Use of austenitic electrodes will minimize the latter.	May be welded with gas or electric arc, or by resistance. Anneal to reduce embrittlement alongside welds. Use small electrodes and low currents to minimize grain growth.	Eminently suited for all welding methods. Weld does not air harden and is very tough. Weldments subject to severe corrodents require post-annealing unless very low carbon alloys or columbium or titanium stabilized types are used.
Corrosion Resistance Increases with chromium content; inferior to Groups B and C. Resists weather, water, steam, and mild corrodents when chromium exceeds 11.5%. High carbon types (over 0.15%) should be used after hardening and stress relief (below 800° F.).	Better than Group A. Excellent for interior trim. Resistant to many chemicals and food products. Extremely resistant to nitric and other oxidizing acids.	Generally better than Group B, especially in reducing and organic acids. Molybdenum aids resistance to sulphite liquors and pitting attack in chlorides. Resistant to most ordinary corrosive agents in domestic and industrial use.
Scaling Resistance Increases with chromium content. Generally useful for continuous service up to 1200° F., and in some atmospheres up to 1500° F.	Excellent resistance to destructive scaling up to 2100° F., depending on chromium content. Resistant to oxidizing and reducing gases. Presence of sulphur lowers maximum useful temperature.	Excellent where high strength is required in combination with resistance to destructive scaling. High silicon improves resistance to scaling. High chromium, low nickel types resist sulphurous (oxygen-free) gases best.

BITS AND PIECES

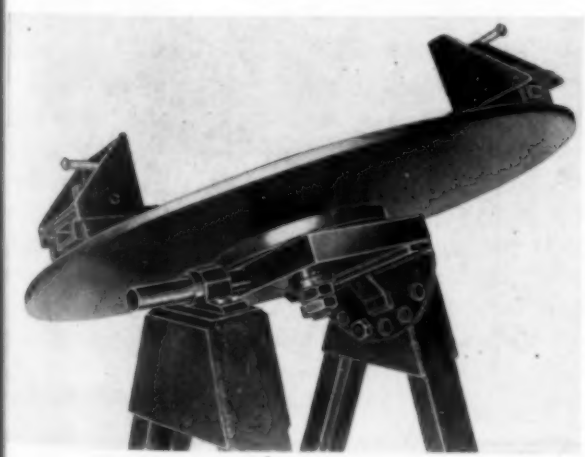
Quenching Oils

IN "BITS AND PIECES" last December, METALLURGICUS signed a brief article on the above subject in which he presented his experience in industry, namely, that plain mineral oil has sufficient quenching speed for nearly all jobs when proper attention is given to the quenching fixtures, agitation, temperature, contamination, and so on. In fairness to the producers of fast quenching oils, similar space and prominence of position should be given to a rebuttal. Such a one has been received from Messrs. WESCOTT and VOLLMER of Gulf Research & Development Co.; it is long enough to be treated as an article, and will be found on page 935. Interested readers may also wish to turn back to page 508 in the March issue, and read a brief argument by "PETROLEUMENSIS".

THE EDITOR

Welding Positioner

"HOME-MADE" welding positioners, used for medium and large jobs at the East Pittsburgh plant of Westinghouse Electric & Mfg. Co., have increased production and improved quality. These positioners can readily be fabricated with a minimum of machining, and require no lubrication in operation. The photograph shows one



unit which has almost unlimited application. We have used it for fabricating axles for railway mine locomotives, blower blades, lead boxes for hydrogen generators, pull-rod boxes for circuit breakers, transformer connections, or any circular or rectangular part weighing up to 500 lb. with dimension of 30 in. max. Heretofore it was necessary to tie up jib cranes or large power-driven manipulators to handle work of this size.

A protractor plate attached to the underside of the 40-in. diameter table allows a tipping range from horizontal to 60° on both sides or a total swing of 120°. The engraving shows the table at the 22½° position, where it is held by a bolt through the protractor plate and the plate on the legs; for the 60° position, the table is tilted till it rests against the legs.

The table, which will revolve while at any angle, can be adjusted to tightness by means of a locknut. Clamps that slide in four T-slots arranged at right angles on top the table allow the work to be adjusted in the most favorable position. The work can usually be centered so the locknuts do not need to be tightened very much; in this condition the table can be rotated by hand to keep any circular work in its best position for down-hand welding. (V. B. KUZMICH, Welding Shop, Westinghouse Electric & Mfg. Co.)

Small Refrigerator for High Speed Tools

HERE'S AN IDEA for shops that cannot afford a refrigerating outfit, yet wish to take advantage of sub-zero treatment of high speed tools.

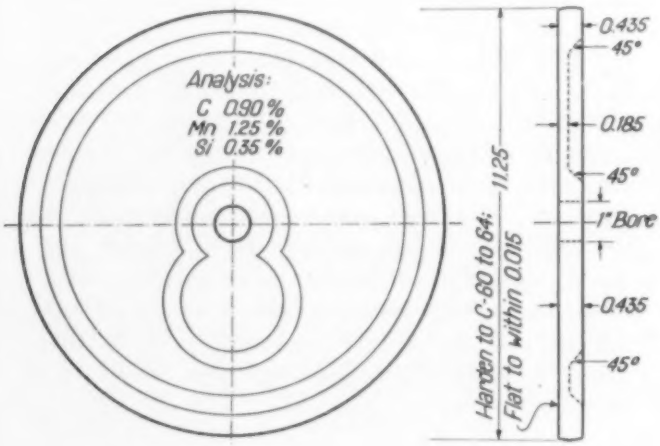
Get a clean metal drum, about 2 ft. diameter by 3 ft. high, a water-tight sheet metal container approximately 8 in. diameter by 18 in. deep, and a cover. Put some insulating compound such as silocel in the drum, place the container in and pack around the sides with more insulation.

To use, fill the container with alcohol, add dry ice, place tools in the container and cover same. The temperature is —100° F., approximately, depending on the amount of dry ice used.

Tools may be refrigerated for 1 to 1½ hr. and then placed in alcohol at room temperature until they warm up. They are then ready for use, and our shop records indicate a noticeable increase in average life in such tools as have been refrigerated in this way, subsequent to the regular heat treatment. (Pvt. BERNARD ZYMEWSKI, Ordnance Dept., Watervliet Arsenal)

A Precision Quench With Inexpensive Fixture

A HEAT TREATER'S NIGHTMARE in the form of a recessed disk came into existence recently in our shop. As shown by the sketch, it was a steel plate, 11¼ in. diameter by 0.435 in. thick. One face of the plate was dished out to a depth of 0.250 in., leaving a flange ¾ in. wide around the outside diameter, and a cam section in the center 0.435 in. thick with a 1-in. central hole.



The analysis of the steel was 0.90% carbon, 1.25% manganese, and 0.35% silicon; it was to be hardened to a Rockwell of C-60 to 64, and a maximum distortion allowance of 0.015 in. No grinding stock was allowed! The pieces were not made in enough volume to warrant the purchase of a straightening press or salt quenching equipment, so the following fixture was made:

We took two mild steel plates, 12 by 16 in. by 1 in. thick, and drilled four corner holes and one center hole for ⅞-in. machine bolts. These plates were surface ground flat to within 0.001 to 0.002-in. tolerance.

The heat treatment on the piece was:

1. Heat to 1000° F. and air cool to relieve machining stresses.

2. Preheat to 1000° F.; high heat to 1450° F. in an atmosphere controlled furnace. Hold 20 min. after the piece is at heat.

3. Quench in oil to 600 to 650° F., which is past the nose of the "S" curve for the steel. The steel is now plastic.

4. Bolt the hot piece between the ground plates, quickly and tightly.

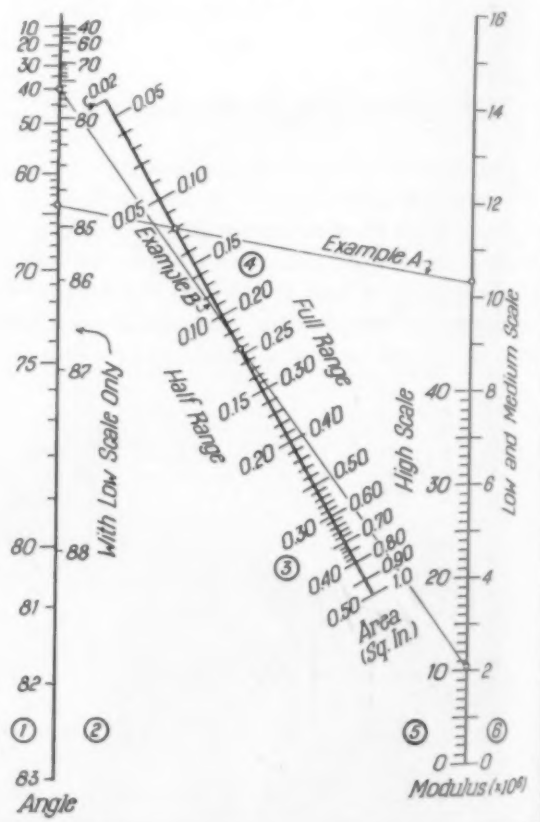
5. After the fixture and the piece have cooled to 175° F. draw the assembly at 350° F. for 2 hr.

6. Re-draw entire fixture at 350° F. for 2 hr.

Resulting hardness was Rockwell C-60 to 62, and the distortion was 0.003 to 0.005 in. About 100 of these plates have been treated in this manner with very satisfactory results. (THEODORE F. BURCH, Supervisor of Tool and Die Heat Treat, Tube-Turns, Inc.)

Chart to Check Accuracy of Automatic Load-Strain Diagrams

ALTHOUGH an automatic tensile testing machine and recorder may have high accuracy and sensitivity, other factors, such as jaw and extensometer slippage or excessive rate of loading, will shift the diagram from its true position.



Modulus of Elasticity of Tensile Test Specimens as Related to the Slope of Autographic Load-Strain Diagram (Elastic Portion) on 60,000-lb. Baldwin-Southwark Testing Machine

causing errors in yield strength determinations. To check the accuracy of load-strain diagrams, Young's modulus should be calculated from the graphs. For this purpose it is convenient to use an alignment chart, such as shown herewith. It serves a two-fold purpose: *(a)* It checks the accuracy of the recorder diagrams in a fraction of the time normally required, and *(b)* it determines the modulus of elasticity from accurate diagrams of materials ranging from magnesium alloys to steels. Required is an accurate protractor to measure the angle between abscissa and the straight line portion of the load-elongation diagram. The chart presented is for a 60,000-lb. Baldwin-Southwark machine. Two examples are solved by construction lines.

Example A

A specimen of 24S-T aluminum alloy plate, minimum cross-sectional area 0.120 sq.in., was tested on the "Medium Full Range" of the machine. The angle of inclination was 64°. *Check:* — Connect 64° on the *first* scale with 0.120 on the *fourth* (Full Range) to obtain the value 10.3×10^6 on the *sixth* scale.

Example B

Another test piece, same size and alloy, was tested on the "Half High Range"; the modulus line sloped at 40°. *Check:* — Connect 40° on the *first* scale with 0.120 on the *third* (Half Range) and read 10.5×10^6 on the *fifth* scale.

Similar charts can of course be prepared for other equipment. It will be recognized that the alignment chart is of the type $X = YZ$, relating three variables. These are θ, the angle of inclination of the elastic portion of load-elongation diagram; a, the cross-sectional area of the tensile specimen; and E, the modulus of elasticity. The constants are a function of the machine and the coordinate paper of the recorder: f is the ratio of the measured distance of a vertical division to a horizontal division; s is the scalar value of one main vertical division; m is the strain magnification of the instrument, that is to say, the overall elongation magnification multiplied by the gage length.

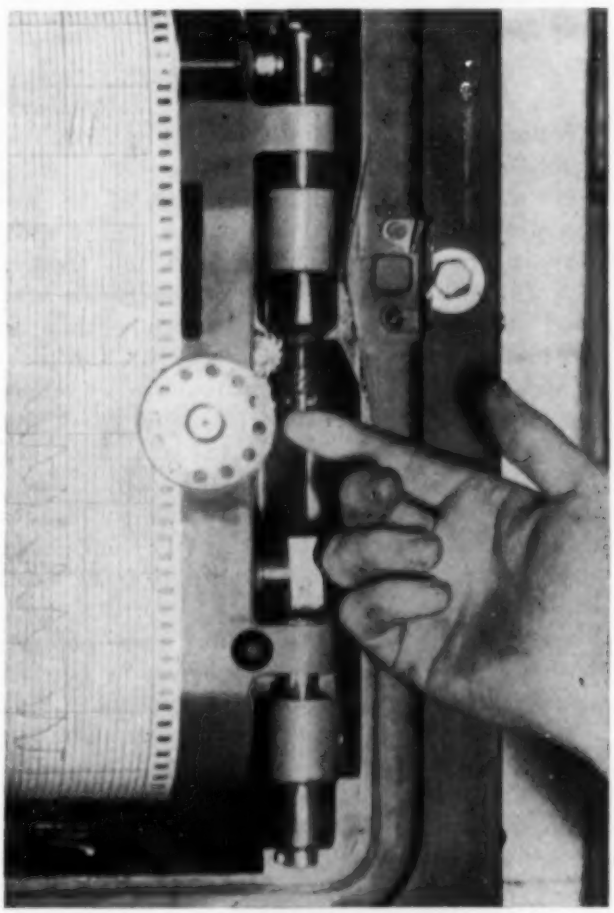
The derived general equation is

$$\tan \theta = \frac{f\,a\,E}{s\,m}$$

We have found use for another chart also, wherein θ is plotted against a for a modulus of 10.3×10^6 (the average for wrought aluminum alloys). This gives us a family of six curves, one each for the high, half high, medium, half medium, low and half low ranges of the 60,000-lb. machine. (AMIEL GELB, Chief Testing Engineer, Reynolds Metals Co.)

Saving Recorder Paper When Furnace Is at Constant Temperature

WHEN A FURNACE controlled by the popular Leeds and Northrup "Model S Micromax" is standing by or is controlling for a long time at constant temperature, it is frequently desirable to stop the chart without affecting the indicating and controlling actions. This is easily done by loosening the set-screws of the helical gear on the lower end of the shaft on which the chart-mount hinges, lowering the gear about ½ in. on the shaft, and tightening the set-screws again.



Once the above change has been made, it requires only a push of the finger to start or stop the chart by sliding the shaft up or down in its bearings, thus engaging or disengaging the attached helical gear. The accompanying photograph shows the gear in the "Chart Stopped" position, and an attendant about to push it up into the "Chart Operating" position. If the chart is to be stopped for an extended period, the pen should be lifted from the paper by placing some thin object between the pen carrier and the bar on which it rests. During the past few years hundreds of feet of chart have been saved in the writer's laboratory by this device, and the records correspondingly condensed.

On earlier L & N recorders, the chart may be stopped by loosening a holding screw and swinging the worm away from the gear on the paper roll. (KELVIN SPROULE, Research Metallurgist, International Nickel Co. of Canada)

Antiseptic Cutting Oils

SKIN INFECTIONS (stubborn dermatitis, septic wounds, and distressing local and general infections) have so frequently resulted from the cutting oils used in machine shops that it is worth a word of warning. Too many of the cutting oils in use have not been manufactured with aseptic precautions, nor sterilized, nor treated with antiseptic agents; they contain germs and fungi and can become more dangerous while in use, because they come in contact with and pick up all kinds of pathogenic agents normally found in a factory, on the benches, the metal parts, the hands of the workers and in the air. The fact that these oils are mixed with metal filings insures a good penetration of the pathogenic organisms through the skin whenever their sharp edges cause a cut, often unnoticed, of the skin. These microscopical cuts are sometimes more dangerous than the more serious ones, because there is no bleeding, and consequently no evacuation of the invading germs or fungi.

The first step in preventing contamination was the sterilization of the oils in an autoclave. However, this was merely a guarantee given by the manufacturer that the oil when sold did not contain any germ or fungus. As soon as the container was opened, the state of sterility ceased. This is why the only way of preventing contamination is the addition of a small quantity of a good antiseptic.

These words "good antiseptic" can be defined by various requirements, the most essential being as follows:

1. When mixed in the oil, the mixture should not harbor and should be able to kill bacteria and fungi within the times specified by the U. S. Department of Agriculture's standards.
2. It must not be a skin irritant.
3. It must be non-toxic.
4. It must not stain the skin.
5. It must not have an offensive smell.
6. It must be directly and permanently miscible in the cutting oils.

Unfortunately several so-called antiseptic cutting oils have been promoted which lacked the above minimum requirements, and many production men have discouraged further trials. This laboratory has tested many antiseptics in years past and can vouch for the fact that several satisfactory ones *are* available, and plants which are using them have been able to reduce absences from skin infections almost to the vanishing point. (LOUIS C. BARAIL, Chief Bacteriologist, United States Testing Co.)

Identification of Plastic Mounts

VARIOUS METHODS have been suggested in recent issues of *Metal Progress* for identifying metallurgical specimens mounted in plastics such as lucite or bakelite. I have found that an ordinary pen will work. Waterproof drawing ink will not "wet" a smooth plastic surface, but if 2 ml. of ethyl alcohol is added to an ounce bottle of draftsman's india ink, the alcohol serves as an efficient wetting agent. A photograph of one of these mounts is attached. (H. F. BARTELL, Materials Laboratory, Wright Field)

THE EFFECT OF TOLERANCES ON MEASUREMENTS

By Sir Charles Darwin
Director
National Physical Laboratory
Teddington, England

The tolerances that may be admitted in manufacturing processes have always been an important matter in works practice and to inspectors, but they have not been a subject for deep study, but rather a nuisance which would not arise in a perfect world. Recent developments in theoretical physics have altered this view, and have shown that the subject of errors and tolerances is not only interesting in itself, but is important in the foundations of physics. Sir Charles uses some homely examples to show what the limits of physical measurements can be—their ceiling of accuracy, so to speak.

MAKERS of almost any kind of mass-produced article are very familiar with all the troubles that arise in working to assigned tolerances. I expect for the most part they regard it as a nuisance, and that in a properly regulated world it is the sort of thing that would be left out. After all we none of us like dwelling on our own imperfections, so why, when we have said that we are going to make something an inch long, should we have to waste time over those little peccadillos which make it a few ten-thousandths wrong?

The physicist has been even worse in this respect, because he was always formulating exact "laws" and then trying to verify them experimentally. Of course, he had errors in his experiments, but they were mostly just a nuisance, and he was usually not interested in them in themselves, but only as an impediment to the realisation of what he thought of as an absolute truth. For his purposes he was often quite right in this, but it led to a general outlook from which errors and tolerances were excluded.

Teachers in engineering courses were still worse in this respect. Just as our theological teachers will try to prescribe what will constitute perfect virtue, and will not devote much time to calibrating the various degrees of vice that are inherent in all human conduct, so the teacher in an engineering school describes only perfection, and has not given much attention to the faults which in fact always occur in manufacture. Teaching has tended to be entirely intolerant!

Only three or four years ago I put this to the proof when visiting one of our leading engineering schools, and I asked a teacher what the students were taught about tolerances. He looked a bit blank and said, "I don't know; let's have those two boys back who have just gone out and ask them." So the students (who were second-year scholars) were asked what the word "tolerance" meant. One simply did not know. The other gave the correct answer, but on cross-examination I found he was really cheating,

because he happened to be the son of a prominent engineer and had learnt about it at home.

My main purpose now is to emphasise the point that tolerances have recently become a respectable part not only of engineering practice, but also of fundamental science, and that a study of their principles can be a really interesting subject. You naturally think at once of the great developments in the field of quality control of ordnance and munitions, a subject which was pioneered in the last decade or so by the manufacturers of electric lamps and telephonic apparatus. I am not going to discuss this aspect; to do so would involve going rather deeply into technical mathematics. I would only say that the point of it really is that once you recognise that there *are* to be errors you find that these errors do have very prevalently a certain distribution; large ones become rarer than small according to a rather well defined rule, and you can exploit this rule so as to get a great deal more out of your measurements than you might expect to at first sight. However, this falls outside my subject just now.

Tolerances for Whole Numbers

My first example as to expected errors is with regard to whole numbers. Here is a case where you might think there would be *no* error. There are either 100 or 101 men in a church listening to a sermon, and there cannot be both. But it is very likely that, when the number is large, someone will have come in late and someone else will have to go early, so what is the answer then? One interesting example of the tolerance of large numbers is the rate of accidents — for example, the number of people killed per year in motoring accidents. It used to be at a rate of about 500 per month in Great Britain. What fluctuation would be expected? The law of chance immediately answers that it will be about the square root of this, that is to say, 23, and anything like a fluctuation of 50 either way is distinctly unlikely to happen more than about once in two years, as long as the conditions of traffic remain about constant.

Notice from this that we can say with considerable confidence what month is the safest one to live through: It is February, because that is three days short in 31 — that is, 10% — so there should be, on the average, 50 fewer killed, and this defect is very unlikely to be beaten by chance fluctuation. If you examine anything which depends on whole numbers, you will nearly always find that if they are larger than two figures, the numbers are not exact, because there is nothing essential about their exactness.

My next example is interesting to me personally because it happened to be the thing which suggested to me about 12 years ago what a fascinating subject this was. Before that I had only thought about it in connection with ultimate physical theory. I happened to be visiting a works where was being made a machine for weighing bullets. The bullets were to be used for testing of armor and a rather high accuracy was asked for.

The machine was much like the beam balances used in the mint for weighing coins, but as it had to handle a great many bullets the speed of operation was important. It worked like this: The bullets were pushed in a row along a groove. At one place each in turn came onto a balance with a proper counterpoise and a lever freed the balance. If the bullet was right the balance would stay level, and the next push would send it straight on. But if the bullet was too heavy or too light its support would fall or rise and the push would throw it aside.

The question then is, "How quickly can the balance be made to work?" Obviously, if a very close tolerance is needed, a bullet which is just to be rejected will be only a little wrong, and so will tip the scale very sluggishly. If you assume a tolerance of one part in a thousand, it becomes a very straightforward problem in elementary mechanics; with the given value of gravity, and with the inertia which the system must have because of the bullet itself and the counterpoise, one can easily compute how long it will take to rise or fall to a height equal to the diameter of the bullet. If one attempted to discriminate earlier than this time, the guide for the correct bullets would get in the way of the rejected ones.

The answer came out despite anything one could do; the bullets could not be weighed faster than about one a second. (I ought to say that here, and in most of my subsequent argument, I am going to practise a type of arithmetic which has attractive features for most of us, because I do not pay any attention to mere things like 100% changes! When, therefore, I say that "you can't do more than one a second", what I mean is that you might do two a second, or you might possibly only be able to do one every two seconds, but that you certainly could not do ten a second, and certainly could easily do one in ten seconds.)

The moral of this example is that it takes time to do a weighing, and that the more precise the weighing is to be the longer it will take. It is easy of course to imagine machinery by which, at the price of a good deal of complication, the

process could be speeded up. For example, a long boom might project from the balance midway between two contacts, close together; then a much smaller motion of the balance than the diameter of the bullet would show which way the scale was going. I could also imagine a grab coming out and knocking away the rejects. I do not say this would be a practical machine to make; indeed, it would certainly be simpler to make ten of the simple machines and work each of them more slowly. The point is that we can *imagine* ways of speeding up the business beyond our first estimate. I mention this because later on I am going to come to an expression showing the absolute limit of time that is essential for weighing anything to an assigned accuracy.

My next example is from the theory of sound. Briefly, the question is this: A man sings a note; how long is it before one can say whether he is in tune or off key? To define the experiment we might suppose that a tuning-fork has been struck, and that he is trying to sing the same note. Since a musical note is a harmonic vibration, the voice and the fork are both sending out a train of waves of a certain frequency. If the voice is out of tune its frequency will be a little wrong, which means that whereas at first the maximum of air pressure coming from the singer's throat was in step with that of the fork, after a certain number of cycles it will have got out of step.

When a piano tuner wants to get two notes in tune he listens to the "beats" between them, and gages the accuracy by the seldomness of the beats. He certainly needs to hear two or three beats before he can say if they are tuned — but for the sake of our argument we will be generous and imagine that a single beat would suffice. Thus we can only say the man is out of tune with the fork if there has been one beat between them; that is to say, if there is time for the man's sound wave to put in one more oscillation than the fork has done.

Now let us come to numbers. The man is to try to hit the C below middle C, and we will not criticise him if he is nearer than a semitone to it. This C has a frequency of about 120 cycles a second, and a semitone is about 1/20 in frequency. So we will pass anything between 114 and 126 cycles as our tolerance. The difference of six cycles, plus or minus, means that after a sixth of a second the man will have completed one cycle more or less than the fork; that is to say in 1/6 sec. there will have occurred the one beat we are looking for. So that answer is that the man can sing for a sixth of a second before we can find him out!

A woman has a much harder time. Say she is singing the upper C. This has four times the frequency, and therefore a semitone, again a twentieth, is now 24 cycles different. She therefore gives herself away in 1/24 sec. It is very easy to verify what I have been saying. If you play a jig on the piano and do it in rather deep bass the whole business seems to become meaningless. This is a matter of pure physics (though no doubt the character of the human ear reinforces the trouble), in that the notes struck are really indefinite, because each note is not given time to define itself.

Limits for Resolving Power

Next, take an example from optics, the idea of "resolving power", which was so well worked out by Rayleigh about 70 years ago. The principle is most important in connection with lenses, but there is a simpler case which gives the whole gist of it.

When we want to use a mirror galvanometer for very sensitive measurements we set the source of light and the observing telescope both a long way off, so that a very small deflection of the galvanometer needle gives a large motion of the image. At first sight it would be tempting to think that we could increase the sensitivity indefinitely by increasing the distances, but this is not so, for consider how the image is formed: We will suppose an appropriate lens is put somewhere in the path to focus the filament on a scale. The bright spot on the screen is at the place where all the rays from the lamp have exactly the same number of wave lengths to the screen from every point of the mirror. Now suppose the mirror turns a little, but think of the same place on the screen. We shall expect to detect the deflection when this point first goes dark. When will that be?

The answer is "When the rays that go from the light via one edge of the mirror differ by about a wave length from those that go via the other edge." Light rays from the intermediate parts of the mirror will then arrive on the scale in phases in between the two limits, and the whole lot will cancel out. Thus the smallest turning one can detect is when one edge of the mirror has advanced a wave length compared to the other. It may happen to be convenient to do this by setting up the screen a long way off, but it is just as good to use a more powerful telescope to look at it from near by — or even, as an extreme case, to take a microscope and watch the mirror edgewise itself, for a good microscope can detect just one wave length of light.

So the result that emerges is that it is the

breadth of the mirror that matters, and not the distance at which the screen is set up.

A wave length of light is pretty short, but the principle that we cannot "see" detail smaller than that does not satisfy us, and we want to know whether the finer details cannot be "observed" in some other way than with our own eyes. One obvious trick well known to metallographers is to use a shorter wave length, both by oil immersion and by using ultra violet light. The electron microscope does the same sort of thing to a higher degree, and because it has attracted attention recently I might digress a little to say something about it.

Both electron and optical microscopes work on much the same principle. Let us suppose we want to study the shape of a small black dot. In either equipment we first must have a condenser to throw rays of light (or of electrons) on the field of view. To get good resolution calls for a strong condenser, that is to say, it must collect rays coming in over very wide angles. The ones that fall on the dot (the object) are stopped, but the rest go through, and the microscope lens focuses them on to a photographic plate — or possibly the focal plane may be viewed with an eye-piece. The lenses that bend the electrons are either electric or magnetic fields, without of course the smooth surfaces of glass lenses, but they work in much the same way and all the elementary formulae of geometrical optics apply to them, including the ordinary relationships of distances and focal lengths to magnifications.

However, we are now interested in knowing how small a thing we can "see", and that means that we must find out what is the highest "resolution" we can get from the equipment. Resolution of an optical microscope depends on the wave length of the light, and we must ask what corresponding thing applies to the electron. It was discovered only about 20 years ago that this was a question that had any meaning; here I can only say that the resolution is proportional to the voltage of the electrons, and for the voltages used, such as 60,000, it is about 10^{-8} cm. We might therefore be able to "see" single atoms, because that is known to be about their "size". Unfortunately there is a catch in it.

The resolution of a microscope depends on the angles through which the rays are bent, and for the highest class of microscope the extreme angle should be nearly a right angle. This calls for a very elaborate lens system in order to correct the various aberrations. No one has yet succeeded in correcting any of the aberrations of an electron lens at all (and theory suggests doubt as to whether it will ever be possible to do so to any considerable extent); in fact if the rays in the electron microscope are deflected more than a degree of arc the image is spoiled. The consequence of this is that a great deal of the resolving power is sacrificed, and the most hitherto gained over the optical microscope has been a factor of about 50 in magnification, instead of about 2000 as simple consideration at first suggested. One might hope to improve things by putting up the voltage, so as still further to shorten the wave length, but another trouble then comes in, because the electrons pass so easily through the object that the image becomes too faint to see.

When we want to get down to seeing smaller sizes we know we need shorter wave lengths, and this suggests the use of X-rays. This has already been done long ago indirectly, but no one has yet made an X-ray lens. However, X-rays are exactly the same thing as light, so we may legitimately imagine we could do this. Such an X-ray microscope would take us to about 10^{-9} cm. Use of gamma rays would take us a good bit further, and after that we might imagine we could use the cosmic rays — perhaps with a little hesitation, because they are still rather mysterious. In this way we might suppose we could see things of the size of about 10^{-13} cm., and it might seem natural to ask, "What next?" But the whole subject received a twist about 20 years ago in a different direction, and this I must now describe.

During the first 25 years of this century the quantum theory was a disturber of the peace in the ideas we had about ultimate mechanics, and the continuity of all actions. You will recall that this theory states that in the emission or absorption of energy by unstable atoms or molecules, the process is not continuous but takes place in steps, each step being the emission or absorption of a definite small amount (quantum) of energy. This theory was so obviously right and yet it so hopelessly contradicted all the laboriously compiled (and also obviously right) ideas of the older physics. The conflict between the two was increased about 1922 by an American physicist, Arthur Compton, who first propounded a theory and then verified it experimentally. He asked the question, "What happens when a light wave passes over a free electron?" The old answer would have been, unhesitatingly, that it shakes it to-and-fro a little, and in consequence the electron scatters a little of the light, steadily, all around.

"Not so," said Compton, "according to the quantum theory." Though light has wave properties, it also has qualities that suggest it is made up of a stream of particles, and his guess, from

general analogy with other parts of the quantum theory, is that this is a case where light will be like a particle. Therefore when the light passes the atom there will be a certain chance that the two will behave like a pair of billiard balls. If they miss it is no carom; if they collide the electron will suddenly be thrown off in one direction, and the light will bounce off into another.

To prove the point it would be a good idea to get a good large effect. To do this you must use a "light particle" with considerable mass — a heavy particle, so to speak. Ordinary light is not "heavy" enough, but X-rays will be. So the Compton experiment had to be done not with light but with X-rays, and with these he brilliantly verified it. There are a great many interesting things about this effect, but the only one that I need is that, when light passes over an electron, if the electron scatters the light then the light also scatters the electron — that is to say, it gives it a flip in some direction. This flip is larger the "harder" the X-rays — that is to say, the shorter their wave length.

In the year 1926 the solution of the main contradictions between the old theories and the quantum theory was found. A number of people were concerned, but chief among them was a young German physicist named Heisenberg. At first the solution was in a rather abstract mathematical form, but it was not long before Heisenberg himself discovered a way of understanding it physically.

He asked the question, "What happens if you really start trying to decide *where* an electron is?" You might go about it by making a microscope, but to get satisfactory precision it would require an X-ray microscope because of the short wave length. True, we don't know how to make one yet, but it is doubtless a legitimate thing to imagine. With this X-ray microscope you look to see where the electron is — that is to say, you shine X-rays on the electron, which scatters the light and this scattered light you then focus in the microscope and look at the image.

But now comes the catch: You won't see the electron unless it has scattered the light, but if it does scatter it then it has suffered the Compton effect and has been flipped away. So you do not see where the electron *is*, only where it *was*. To weaken this trouble you might go back to ordinary light, for which the Compton effect is very small, but in doing so you have given up the idea of fixing the electron's position so accurately, because you are using a longer wave length for your microscope, and in a microscope you can never see a position with accuracy better than a wave length.

All this is expressed in the Uncertainty Principle, that it is impossible to fix both position and velocity of a body with unlimited accuracy. There is no limit to the tolerance you may demand for either the position *or* the velocity of a body, but if you make exacting demands on one, that automatically makes you relax your demands on the other.

Notice the new twist the subject has got: There is nothing wrong with the conception that the electron has a definite position and velocity, but it is not a useful idea because you can never find them both out at the same time. In the very act of trying to do so you spoil things and ensure that they will be different from what they were before!

When the theory is worked out in detail the answer can be expressed very easily in terms of tolerances. Suppose the experimenter measures the position in centimeters and the velocity in centimeters a second. Then the most accurate tolerances that can ever be obtained obey the rule that the tolerance of the electron's position, Δx, multiplied by the tolerance of its velocity, Δv, obey the rule,

$$\Delta x \cdot \Delta v = 7 \text{ cm}^2/\text{sec}.$$

I ought to mention that a similar rule holds for other bodies besides electrons, but the number on the right is inversely proportional to the mass of the object; therefore, since the electron is the lightest body existing, it is the one for which the effect is most marked. Since the mass of the electron is about 10^{-27} g. you can see that the effect will not cause serious trouble when measuring ordinary bodies at ordinary velocities.

The Uncertainty Principle is quite fundamental in modern physics. Its effects are inescapable, so that we can see how the idea of tolerance has got to be accepted as one of the basic ideas, and can no longer be fobbed off by saying that with deeper knowledge we could get rid of it. It is the Uncertainty Principle that has finally made tolerance respectable.

There are other ways in which it can be expressed, which from the theoretical point of view are much more powerful, but it is not necessary to go into them. I shall only take one further rather different example of it, because

it will connect up with one of the earlier examples, that of weighing the bullets.

In Heisenberg's example, fixing the position spoiled the velocity, which we express by saying that position and velocity are complementary. In the same way, mechanical principles show that energy and time are complementary. Now the principle of relativity shows that energy and mass are the same thing, and therefore the Uncertainty Principle asserts that it takes a certain time to determine a mass with a given precision, and that the smaller tolerance we demand in the mass the longer we must take in doing the weighing.

Maximum Speed of Weighing

I shall not attempt to design and explain an experiment to show how this comes about, nor shall I go through the calculations, but I will give you the result only: Suppose our bullet weighed 10 g., and that we still want it to a tolerance of 1 in 1000 — that is, to the nearest 0.01 g. Then you cannot hope to do this in less than 7×10^{-45} sec. This is pretty short, so there is evidently a distinct possibility for improving our present methods, which require about 1 sec.

Are there any further limitations to tolerances in nature besides the Uncertainty Principle? We do not really know, but there have been rather plausible conjectures that there are. For instance, there may be no meaning in talking about a distance shorter than 10^{-13} cm., which is about the size of the nucleus of the atom. I shall not attempt to discuss these rather vague ideas, but, at the risk of an anti-climax, shall turn over to quite a different field where the idea of even finer tolerances is important.

This is the field of pure mathematics. Remember the celebrated problem of Achilles and the tortoise: The swift Achilles, pursuing a tortoise, reaches the spot whence the tortoise started; the tortoise has now moved on to a further spot; when Achilles arrives there, the tortoise has once more advanced — and so on (to infinity?). The answer that Achilles would really catch up the tortoise was just as obvious to the Greeks who propounded the problem as it is to us, but that is not the point. The point is that the ideas of infinity and continuity are mathematically identical, and when the philosophers try to get down to pure logic there are contradictions involved in the idea of a line being made up of a row of points, and the argument can be presented in such a way that it is very hard to see what was wrong in it.

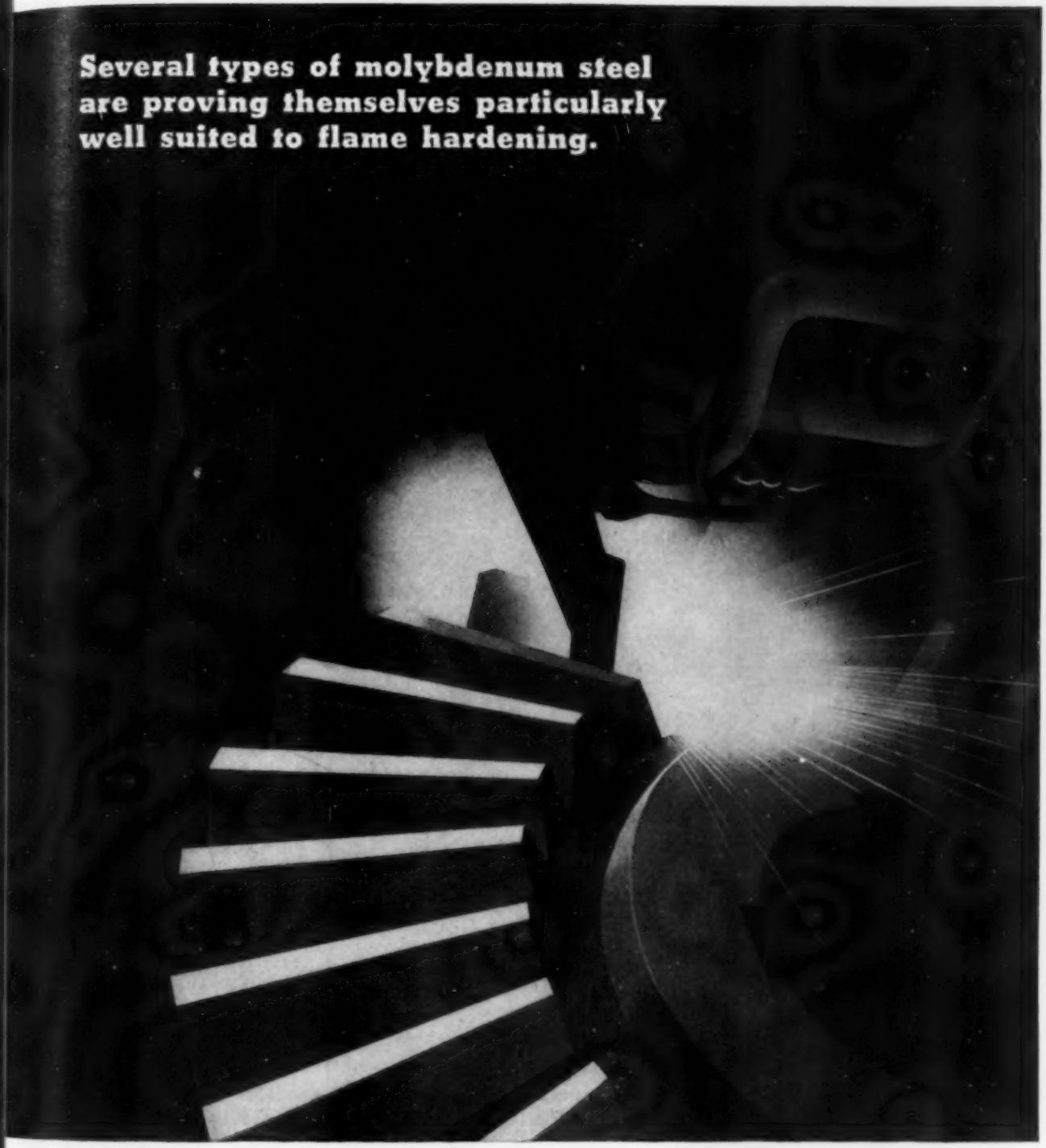
That it actually was hard may be seen by the fact that it took over 2000 years to find the solution. This solution was the act of a number of mathematicians about 100 years ago. Their work, being of a logical kind, had to be worded with extreme care, and if you read the proofs they give of some of their important theorems you are reminded of the way a lawyer draws up a rather complicated deed, putting into it a lot of things you never thought of. In the course of these proofs great use is made of a particular letter, the Greek letter epsilon, ϵ. (I don't know why it is always this letter that is used, as there is no particular reason why it should be, but the subject is sometimes irreverently called epsilonology on the strength of it.) Now the whole thing would have been really fairly simple if mathematicians had been accustomed 100 years ago to the idea of tolerance, because ϵ is nothing but the tolerance.

I am going to give you an example: What do we mean by $\sqrt{2}$? There is no number which, multiplied by itself, gives 2. This troubled the Greek mathematicians a great deal, and indeed they called such numbers "irrationals", unreasonable numbers. In later times it became clear that though there was no such number, one could pretend there was, and play with it in arithmetic as if it were a genuine number. Still it was not a rational or reasonable number, and no one could answer the difficulties of the Greeks.

The correct answer was given 100 years ago in this manner: *You* say there is no such number. *I* say to you that if you will assign any tolerance you like I can give you a number which will have a square nearer to 2 than your tolerance. I don't care how close a tolerance you fix, I can beat it. The way is obvious. If you say a hundredth, I give you the square root to three places of decimals; if you say a millionth, I give it to seven places, and so on.

The arguments about irrationals are not sound, not because irrationals are numbers in the ordinary sense, but because whatever tolerances are assigned, numbers can be found which are closer to the result. I am not going to say that this description would suit the lawyer-like habits of the pure mathematician, but it is not a bad definition of his subject to say that pure mathematics is the branch of physics which is true for *any* and *every* tolerance.

I have now traversed the field of tolerances from large numbers, through objects of ordinary size, then through objects so small that we need a powerful microscope to see them, then to the ultimate physical realities, and now down to anything of completely unlimited size. There is no further to go.



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PERSONALS

Promoted by E. F. Houghton & Co., Philadelphia: D. J. RICHARDS, from assistant director of sales to vice-president, sales.

JAMES EDWARD CAMPBELL, formerly associated with the General Motors Research Laboratories, Detroit, has been appointed to the technical staff of Battelle Institute, Columbus, Ohio, and assigned to the division of physical metallurgy.

WALTER R. GRIFFITH, formerly with the Jacobs Aircraft Engine Co. of Pottstown, Pa., is now chief inspector of the Firestone Tire & Rubber Co., Ordnance Branch, Burlington, N. C.

Honored by the American Foundrymen's Association: CLARENCE E. SIMMS, supervising metallurgist, Battelle Memorial Institute, Columbus, Ohio, awarded the Penton Medal of the A.F.A. for "outstanding work in behalf of the steel foundry industry"; ROBERT E. KENNEDY, secretary, American Foundrymen's Association, awarded the Joseph F. Seaman Medal for "his outstanding and long continuous work on behalf of the foundry industry in general".

FRED J. TOBIAS, formerly production engineer for Sam Tour & Co., Inc., is now vice-president of the A.R.D. Corp., New York.

E. K. SMITH, metallurgical specialist, C. U. Division, U. S. Department of State, is now in China as a member of the Advisory Committee on Iron and Steel, War Production Board, National Government of China.

ARTHUR C. RAE has been sent to South Africa by Atlas Steels Ltd. of Canada for a period of two to three years.

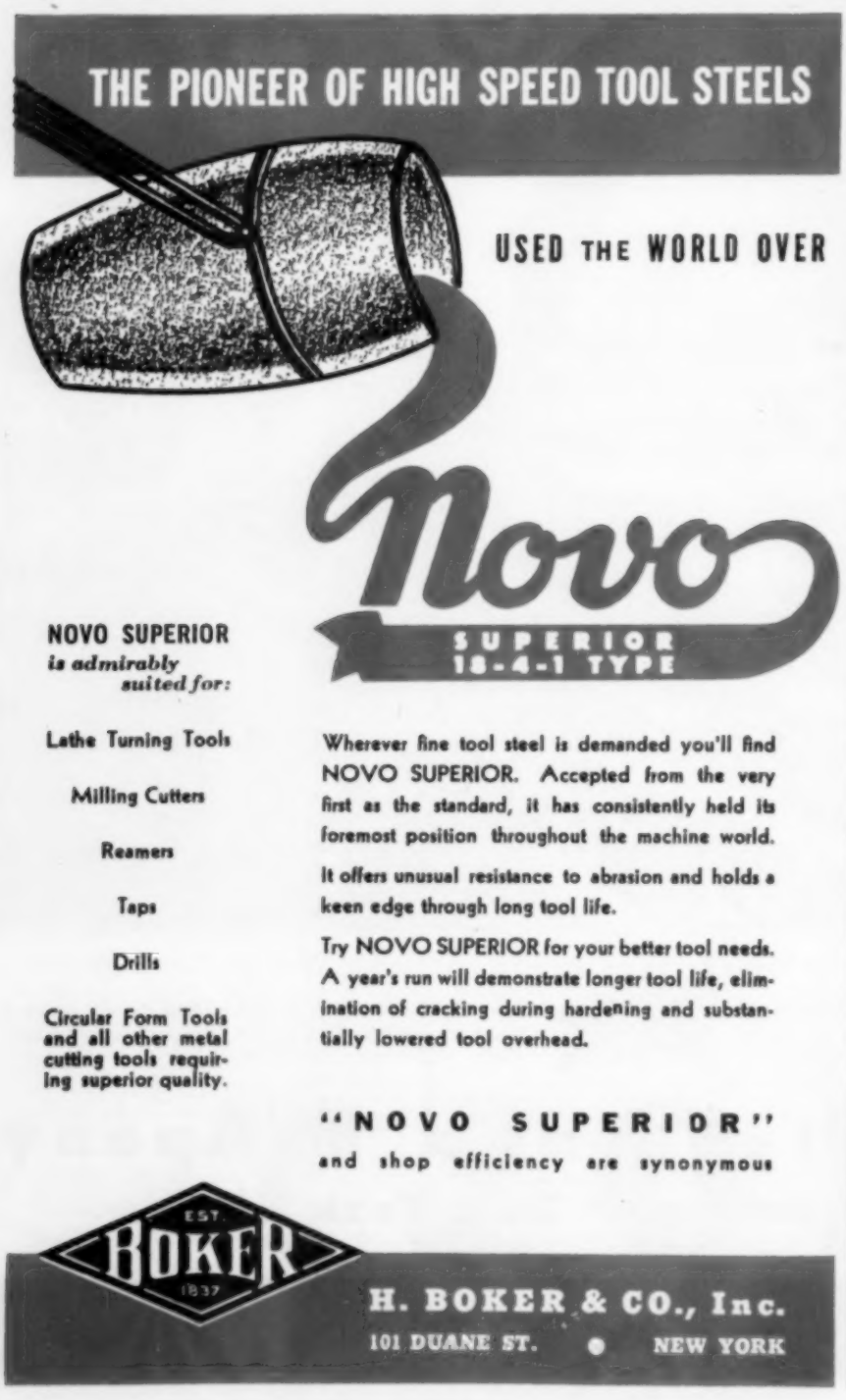
NEILL H. MARTIN is now general manager and factory superintendent of the Leslie, Mich. plant of Mechanical Products Co. of Jackson, Mich.

Honored: ZAY JEFFRIES, vice-president, General Electric Co., Pittsfield, Mass., by the award of the Clamer Medal of the Franklin Institute "for meritorious contributions to the science of metals, which he has placed on a new and more intelligible basis"; also by the first award of the newly established annual Powder Metallurgy Medal of the Stevens Institute of Technology on the occasion of his presentation of the first annual Medal Lecture.

PAUL S. KINGSLEY, formerly chief metallurgist, Follansbee Steel Corp., is now supervisor, vendor's material control, Wright Aeronautical Corp.

Lt. (j.g.) R. M. GARRISON has been moved from Cincinnati, Ohio, to Schenectady, N. Y., where he is affiliated with the Inspector of Naval Material in the plant of General Electric Co.

A. W. HUBBARD, formerly supervisor, manufacture training and coordination, Boeing Aircraft Co., Wichita Division, is now technical director for Electric Machinery Mfg. Co., Minneapolis.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This is oxygen-free, high conductivity copper containing a small amount of tellurium, which greatly increases the speed with which the metal can be machined, makes it possible to hold to closer tolerances, and improves the finish of the completed parts. Electrolytic copper, while suitable for many purposes characteristically machines with long tough stringers which are difficult to remove. Free cutting copper chips are brittle and break readily, clearing from the tools without attention.

These advantages increase production rates, depending upon the amount of machining necessary on each given part. Consequent lowered costs may be an important factor to you.

Revere Free Cutting Copper Rod is available in sizes up to 2" and in all the usual shapes. For details, write the Revere Executive Offices.

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PERSONALS

R. J. HERBENAR, formerly with Timken Steel and Tube Division, is now with Craine-Schrage Steel Division of Detroit Steel Corp.

H. W. ZIELER, formerly with E. Leitz, Inc., is now general manager of Hellige, Inc., Long Island City, N. Y.

CHARLES C. BRAY, formerly metallurgist for the special steel department of the Philadelphia office of Joseph T. Ryerson & Sons Co., has been appointed assistant to the manager of the worm gear division of the De Laval Steam Turbine Co., Trenton, N. J.

FELIX EDGAR WORMSER, secretary and treasurer of the Lead Industries Association, New York, has also been elected secretary and treasurer of the Metal Powder Asso.

HARRY C. FAUST, formerly chief engineer and metallurgist for the Chicago Coil Spring Co., is now a partner in the Paragon Spring Co., Chicago.

TRACY C. JARRETT, chief metallurgist of the American Hammered Piston Ring Division of the Koppers Co., Inc., has been appointed to the Technical Committee of the Gray Iron Founders' Society.

JOHN E. DECKER, formerly laboratory foreman, Wood Works, Carnegie-Illinois Steel Corp., McKeesport, Pa., has been transferred to the research and technology department of Carnegie-Illinois in Pittsburgh as surveyor, quality control division.

CLAYTON F. FISHER has been elected president and general manager of Sleeper & Hartley, Inc., and RAYMOND F. RUSSELL has been elected vice-president and treasurer. WILLIAM H. BLOUNT, former president, has retired.

ALLAN L. TARR, formerly of Basic Magnesium, Inc., Henderson, Nev., is now in charge of research and development, Aluminum-Magnesium Division, Revere Copper and Brass, Inc., Baltimore.

Lt. J. HASKINS HARTWELL, U.S.N.R., is on leave of absence as works manager of Hartwell Iron Works, Houston, Texas, and is stationed at Washington, D.C., in the Aviation Ordnance Division of the Washington Navy Yard.

J. H. G. WILLIAMS has been made general manager of Roxbury Steel Casting Co. in Boston, a subsidiary of the Hartford Electric Steel Corp.

JOHN P. MCCAFFERTY, formerly with the Amalgamated Steel Co., Cleveland, is now employed by Thompson Products Co., Inc., Cleveland, as a tool hardener.

HERLUF P. NIELSEN, formerly chief metallurgist at the Kinney Aluminum Co., Los Angeles, has been appointed to the staff of Battelle Institute, Columbus, Ohio, assigned to the division of non-ferrous metallurgy.

G. KINGMAN CROSBY has been appointed superintendent of production at the Huntington Works of the International Nickel Co., Huntington, West Va.

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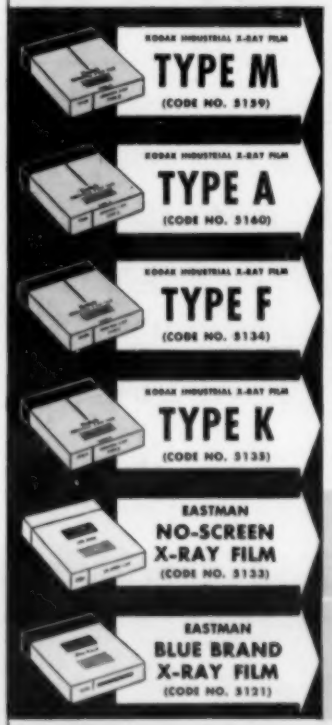
NEW! A helpful chart for Radiographers

FILM CHARACTERISTICS

THE choice of a film for a particular job depends upon the relative emphasis placed upon definition, contrast, and exposure time, and the available kilovoltages or intensity of gamma radiation.



KODAK INDUSTRIAL X-RAY FILM
TYPE M
(CODE NO. 5159)

EXTRA fine grain and high contrast. "Made to order" for critical inspection, higher voltages, and the radiography of light metals such as aluminum, magnesium, and their alloys. Gives highest obtainable quality at 1000 kilovolts. For use direct or with lead-foil screens.

KODAK INDUSTRIAL X-RAY FILM
TYPE A
(CODE NO. 5160)

FINE grain and high contrast. Designed for the radiography of light metals at lower voltages and for 1000-kilovolt radiography of heavier steel parts. Its grain is not quite as fine as that of Type M, but its higher speed makes it more widely useable. For use direct or with lead-foil screens.

KODAK INDUSTRIAL X-RAY FILM
TYPE F
(CODE NO. 5134)

HIGHEST available speed and high contrast when used with calcium tungstate screens. Useful for examination of steel, brass, etc., with machines of limited kilovoltage.
Lower contrast when used direct or with lead-foil screens. Records large range of thicknesses in relatively small density range.

KODAK INDUSTRIAL X-RAY FILM
TYPE K
(CODE NO. 5135)

HIGHEST available speed for gamma-ray work when used direct or with lead-foil screens.
In work with calcium tungstate screens, Type K has about 1/10 the speed of Type F, but has higher contrast and greater density range.

EASTMAN
NO-SCREEN X-RAY FILM
(CODE NO. 5133)

USEFUL where higher contrast than that of Type K is needed; and where somewhat lower speed can be tolerated in gamma-ray radiography. Its high contrast makes it especially suitable for gamma-ray exposures. For use direct or with lead-foil screens.

EASTMAN
BLUE BRAND X-RAY FILM
(CODE NO. 5121)

IN WORK with calcium tungstate screens, Blue Brand is slightly slower than Type F under industrial conditions.
When used direct or with lead-foil screens, Blue Brand is similar to Type F used under the same conditions.

RELATIVE SPEED AND CONTRAST

THE relative speeds and contrasts given below apply only to the use of the film in direct exposures or with lead-foil screens, as indicated.

The *Type F* and *Blue Brand* films are primarily intended for exposure with calcium tungstate intensifying screens; under such conditions, their speed and contrast are much higher than indicated below.

The values listed in column one below apply to x-ray diffraction work as well as to radiography.

	Aluminum and other light alloys No screens	Thin steel and brass Lead-foil screens	With million-volt x-rays Lead-foil screens	With gamma rays Lead-foil screens
RELATIVE SPEED	30	40	30	30
CONTRAST	HIGH	HIGH	HIGH	HIGH
RELATIVE SPEED	100	100	100	100
CONTRAST	HIGH	HIGH	HIGH	HIGH
RELATIVE SPEED	200	200	200	360
CONTRAST	MEDIUM	MEDIUM	MEDIUM	MEDIUM
RELATIVE SPEED	530	400	425	500
CONTRAST	MEDIUM	MEDIUM	MEDIUM	MEDIUM
RELATIVE SPEED	600	400	250	300
CONTRAST	HIGH	HIGH	HIGH	HIGH
RELATIVE SPEED	200	200	200	360
CONTRAST	MEDIUM	MEDIUM	MEDIUM	MEDIUM

DEVELOPMENT

with Kodak Liquid X-ray Developer or Kodalk X-ray Developer (powders)

Normal Development at 68°F. (20°C.) in minutes	For maximum speed and contrast at 68°F. (20°C.) in minutes
5	8
5	8
5	5
5	8
5	8
5	5

APPROXIMATE RADIOGRAPHIC EQUIVALENCE FACTORS

ALUMINUM is taken as the standard metal at 50 Kv.P. and 100 Kv.P., and steel at the higher voltages. The thickness of another metal is multiplied by the corresponding factor to obtain the approximate equivalent thickness of the standard metal. The exposure applying to this thickness of the standard metal is used.

Example: If 0.5 inch of copper is to be radiographed at 200 Kv.P., multiply 0.5 by the factor 1.4 to obtain 0.7, the equivalent thickness of steel. Therefore, 0.5 inch of copper can be given the exposure required for 0.7 inch of steel.

The bronzes may require somewhat different factors than copper or brass, depending on their composition.

Kilovolts	Aluminum	Magnesium	24ST Alloy	Steel	18-8 Stainless Steel	Copper	Zinc	Brass	Lead
50	1.0	.6	1.4						
100	1.0	.6	1.2	22					
150				1.0	1.0	1.5	1.4	1.4	12
200				1.0	1.0	1.4	1.3	1.3	12
1000				1.0				1.2	5
Gamma rays				1.0				1.1	2.3

larger size (18" x 22") in three colors write Eastman Kodak Company, X-Ray Division, Rochester 4, N. Y.

PERSONALS

MILO J. STUTZMAN of the department of engineering research, University of Michigan, has joined the staff of the Midwest Research Institute, Kansas City, Mo.

G. V. LUERSSEN of The Carpenter Steel Co., Reading, Pa., has been awarded the Stoughton Plaque for outstanding achievement in metallurgical research by the Lehigh Valley Chapter.

Appointed to head three new departments of the Development and Research Division of the International Nickel Co., Inc., New York: O. B. J. FRASER, director of technical service on mill products, to head the new Industrial Chemicals Section; F. L. LAQUE, to head the Corrosion Engineering Section; and DONALD J. REESE, until recently with the Steel Division of the War Production Board in Washington, to head the Iron and Non-Ferrous Casting Section.

Promoted by Joseph T. Ryerson & Son, Inc.: W. O. SPRINGER, formerly in charge of the Cleveland Special Steels Division, to manager of the Cleveland Steel Service Plant; CARL C. GOBDEL, formerly sales representative in Missouri, to manager of the special steels department at the Philadelphia plant.

JOHN L. YOUNG, formerly vice-president of industrial research and development of the United Engineering & Foundry Co., Pittsburgh, has been elected to the newly created position of vice-president in charge of engineering of the National Tube Co.

R. L. MITENBULER is now district manager for the state of Wisconsin for Bliss & Laughlin, Inc., succeeding L. E. MEIDINGER, retired.

A. S. RAIRDEN has been appointed sales manager of the wire rope division of Wickwire Spencer Steel Co.

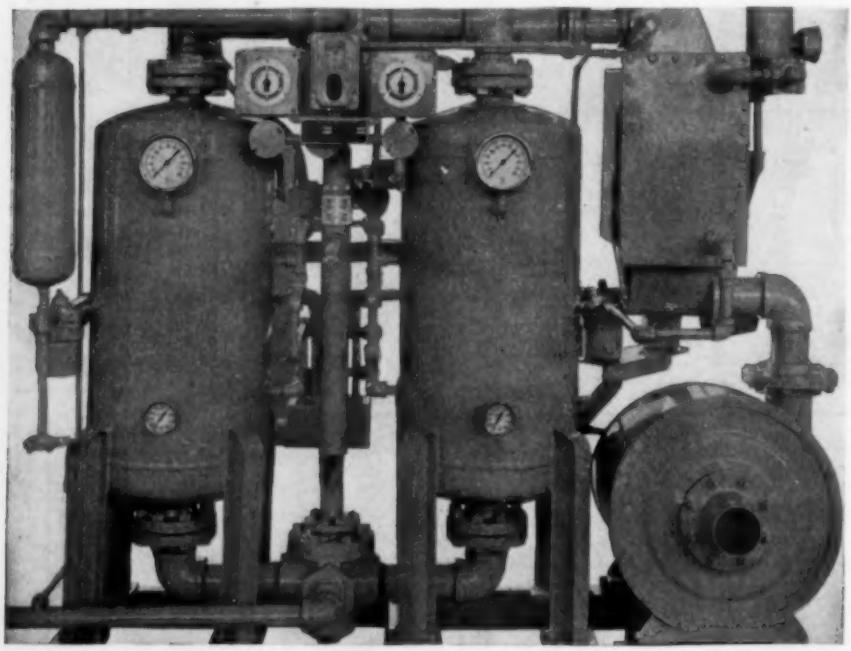
FRANK R. WRIGHT, formerly district manager in charge of the Buffalo office of Rustless Iron and Steel Corp., has been appointed district manager for the New England territory with headquarters in Boston. J. C. NEILSON, formerly in the Milwaukee office, has been transferred to the Buffalo office as a field service engineer.

Appointments by the Youngstown Sheet and Tube Co.: E. E. ELLWOOD, who formerly covered New York State with headquarters at Albany, to assistant district sales manager of the St. Louis district; J. P. FEAGLEY, formerly sales representative in the New York office, to assistant district sales manager of the New York district.

HAROLD N. EWERTZ has been appointed manager of the Philadelphia district for American Machine and Metals, Inc.

Promoted by Carnegie-Illinois Steel Corp.: FRANK T. BUMBAUGH, from assistant manager to manager of sales, alloy division.

W. W. JULBERT is now chemical engineer in charge of inspection, electroplating, and chemical analysis for V. E. Sprouse Co., Columbus, Ind.



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